INDIVIDUAL DIFFERENCES IN REACTING TO DELAYED AUDITORY FEED-BACK

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The speech of normal subjects is impaired when the subject hears himself speak a fraction of a second after uttering, that is, when auditory feed-back is delayed. A study was made of individual differences in showing this impairment. It was found that those showing the impairment in marked fashion were likely to have high scores in tests of other kinds of 'perseveration'.

I. INTRODUCTION

t has been known for some time (Lee, 1951) that delayed auditory feed-back impairs the peech of normal subjects. There are four major effects: (1) a general slowing down of peech; (2) a prolonging of syllables—'illuminated' might become 'illooomminated'; 3) a repetition of consonants—'twittering' might become 'twittittering'; (4) occasional omplete stops or blocking. The induced stutter shown in effect (3) generally occurs when he subject attempts to maintain a natural speed of talking. Effect (4) was described by ne subject as 'allowing my ears to catch up with my voice'. There are two main ways of voiding the stutter, namely, by speaking very slowly, and by speaking in short, rapid ursts. Both methods result in an overall slowing down of speech.

Individual differences in susceptibility to the artificial stutter are so marked and consistent that it seems reasonable to try to relate these individual differences to other haracteristics of the subject. In choosing the tests used for determining these other haracteristics, two 'hunches' were involved. The first was that individuals might differ in heir ability to ignore feed-back, or in their ability to use it in a novel way; this might r might not be independent of the modality in which the feed-back occurred. The econd was that the delayed feed-back interference is related to perseveration. It might e that susceptible individuals would also be susceptible to other kinds of perseveration.

II. EXPERIMENTAL

(i) *Apparatus and tests.* The delayed auditory feed-back was supplied by a modified ape recorder. The subject wore headphones and talked into a microphone. His voice was ecorded on the tape and relayed back to him from a pick-up head which was a small istance further along the tape than the recording head. The delay could be adjusted by ltering the speed of the tape. This was adjusted to give a delay of approximately one-fth of a second. The other tests were as follows: (*a*) Star tracing. This was the familiar est in which the subject has to trace between the contours of a double contoured star hile observing his movements in a mirror. (*b*) Mirror writing. The subject had to write tters and digits normally while observing his movements in a mirror so that the haracters would be seen by the subject in mirror image form. (*c*) Einstellung test. This as devised by Luchins (1942). In it, a set was created to solve simple arithmetical roblems in one, rather complex, way. Later, six critical problems were given in which is same method could be used for solution, or an alternative simpler method could be

used. If subjects persisted in using the original method, they were said to show th Einstellung effect. (*d*) Imagery rigidity. This was a test invented by Gordon (1949). I consisted of a series of eleven questions involving the manipulation of the image of a car e.g. 'what colour is it?', 'can you see it lying upside down?' Subjects with rigid imagery were said to find the manipulation of their images very difficult. That is, once having seen an image of a certain car in a certain situation, such subjects would be unable to see it any other way.

The star tracing and mirror drawing tests were included because they both involved th utilizing of visual feed-back in a novel way; also, they were tests of one kind of persevera tion—habit perseveration. The Einstellung and imagery rigidity tests were also though to test habit perseveration.

(ii) *Subjects and procedure.* The subjects were thirty non-psychologists with an ag range of 18–30 years. The visual tests, followed by the Einstellung and rigidity tests, wer carried out two weeks after the test with delayed auditory feed-back.

Delayed auditory feed-back: the subject was shown the tape-recorder and a brie account of its performance was given. He then put on the earphones and spoke a fe words into the microphone. This familiarized him with the feel of the apparatus and als allowed the initial shock of the effect to pass off. After removing the earphones, he wa given a page of printed matter of 400 words length and asked to read it aloud in hi normal speaking voice. Shortly after he had finished, he was asked to recite aloud tw short nursery rhymes twice each, the total length of utterance being ninety-six words This performance was then repeated under experimental conditions, i.e. with the subjec wearing earphones and hearing his voice $\frac{1}{5}$ sec. after he had spoken. During both contro and experimental performances, the experimenter noted the time for each 50th wor during the reading and the overall time only for the recitation. As a check of reliability nine subjects were given a re-test on the reading, using a different passage, about tw weeks after the first trial.

Manual-visual activity: both these tests were carried out at the same time some thre weeks after the above test. Subjects were required to write letters in disconnected scrip and the digits 1–9 for periods of 20 sec. each alternately. After one period of each unde normal conditions, a screen and mirror were placed so that the subject could observe hi performance only in the mirror. Four alternations of letters and digits were complete under these conditions. The screen and mirror were then removed and a 5-point sta placed before the subject. He was asked to trace its outlines as rapidly as possible withou making any mistakes. This was then repeated with the screen and mirror in place: th usual procedure for mirror tracing. A single control and four experimental runs wer timed, followed by four more alternations of letters and digits and finally, after removal o the screen and mirror, one more control period of each. The numbers of letters and digit written in each period and the times taken for star-tracing were recorded. The Luchin Einstellung test and the Gordon test were administered in the way described by thei inventors.

III. Results

Table 1 shows the mean scores for all except the Luchins and Gordon tests. The las column indicates the effect which the experimental conditions had on performanc either as percentage increase in time taken on a task, or as percentage decrease in amoun

f work done in a given time. No assumptions could be made about the distribution of cores, so subjects were placed in rank order for their performance on each test, and non-arametric techniques were used in assessing contingencies. Correlations were calculated ith Kendall's tau.

(1) *Reliability.* A reliability test was made on the auditory feed-back experiment since othing was known about consistency of performance on it. For the nine subjects used, he correlation was 0·83 with a standard error of 0·27, which was considered satisfactory.

able 1. *Means and ranges of measures used on the visual and auditory tests. Thirty subjects*

	Control		Experimental		Effect	
	Mean	Range	Mean	Range	Mean	Range
	(in sec.)				(% increase)	
Reading						
First quarter	18·3	14–24	28·0	18–39	54·5	−4–155
Total task	155·5	130–198	221·7	155–368	47·1	−4–165
Star-tracing						
First star	15·1	9–26	207·5	42–639	1242·0	250–3790
Average for 4 stars	—	—	121·4	38–279	761·0	218–2150
Recitation	25·1	11–32	44·2	14–88	78·2	17–309
	(Amount written in given time)				(% decrease)	
Writing						
First trial	135·6	94–196	88·0	23–159	35·8	10–79
Average for all trials	—	—	192·5	67–349	29·4	4–70

(2) *Relationships between subtests.* There was a significant correlation of 0·33 between he overall effect on star-tracing and the overall effect on writing. In the auditory tests, here was a significant correlation of 0·56 between the overall effects on reading and eciting. The Gordon and Luchins tests did not correlate.

(3) *Adaptation.* In all the experiments in which feed-back was interfered with, there ere significant correlations between the initial effect of the experimental condition and he speed of adapting to it. That is, the worse the impairment on the reading tests, the ore rapid the adaptation. (The most likely explanation of this is that if subjects were little ffected by the experimental conditions, they were working at something like their ormal speed, so that little improvement could show; whereas if their performance was onsiderably affected, there would be greater opportunity for improvement.)

(4) *Relationships between the auditory and visual tests.* The overall effect on reading rrelated 0·11 (not significant) with overall effect on writing, and 0·30 (significant) with verall effect on star-tracing. The overall effect on reciting was correlated positively, but ot significantly, with the effects on the visual tests.

(5) *The Luchins Einstellung tests.* Table 2 shows the scores of subjects classified accord-g to whether they showed complete Einstellung or not. The scores on other tests of ese two groups were compared using the Mann-Whitney U test (one-tailed), the hypo-esis being that those subjects showing complete Einstellung would be highly perseverant n the other tests. The probabilities, in the last column, show the significant contingencies at exist.

(6) *The Gordon imagery test.* This gave only one significant correlation, which remains nexplained. Subjects who said they were unable to visualize at all were quick reciters nder normal conditions. Statistically this was a highly significant relationship.

Table 2

(Subjects are classed according to whether they showed complete Einstellung or complete lack of it. Th 4 in-between subjects are omitted. The figures are in percentages, as in Table 1.)

	Einstellung ($N=11$)		Non-Einstellung ($N=15$)			
	Mean	Range	Mean	Range	U	p
Reading						
Effect on first quarter	69·6	14–152	44·0	−4–155	55	>0·05
Overall effect	62·0	24–165	25·2	−4–94	37	<0·01
Star-tracing						
Effect on first trial	1730	656–3790	876	250–1615	30	<0·01
Overall effect	967	305–2150	604	218–945	40	<0·05
Recitation	89·9	17–309	63·2	19–158	57	>0·05
Writing						
Effect on first trial	41·7	13–79	32·0	10–55	59	>0·05
Overall effect	33·7	5–70	26·0	4–50	63	>0·05

IV. Discussion

The first 'hunch' tested was that subjects might differ in their ability to ignore feed-bac or to use it in a novel way. The low positive correlations between the auditory a visual effects might indicate that there were such consistent individual difference However, the significant correlation between writing and star-tracing throws doubt this, since, in writing, the mirror image has to be ignored, whereas in star-tracing it has be used in a novel fashion. This suggests that the second 'hunch' is more plausible: th the tests are sampling individual differences in perseveration. The significant conti gencies existing between results on the Einstellung test and the reading and writing te add weight to this argument.

Eysenck (1947, pp. 154–6) has divided the various tests of perseveration into fi classes, and suggests that the correlations between classes are low or negligible. T present study would suggest that two of these classes of perseveration are significan correlated. They might be defined as follows (adapted from Lankes, 1914). First, a hi perseverant is an individual whose performance on a task is more than normally impai by the after effects of previous movements, or previous stimulation. (The stutteri machine—delayed auditory feed-back—assessed perseveration of this kind.) Second a high perseverant is an individual whose performance on a task is more than norma impaired by previously learnt habits which conflict with the present task. (The Einstellu test, star-tracing and writing while looking in a mirror seem to be examples of this kind perseveration. The writing test seems the least good measure, having smaller correlatio since the writing habit can be carried on successfully if the subject ignores what he see the mirror.)

It is interesting that Solomon (1952) discovered that natural stutterers showed m pronounced Einstellung than a normal group and that this agrees with common clini experience that stutterers tend to have 'rigid' personalities. This suggests that peo who are susceptible to delayed auditory feed-back may have something in common w natural stutterers. This is not so surprising in view of demonstrations by Cherry, Saye Marland (1955) that natural stutterers are able to talk at a more normal rate if th auditory feed-back is blocked by a loud masking noise.

The authors wish to express their great indebtedness to Dr D. M. Mackay of King's College, London, who lent them his apparatus for providing delayed auditory feed-back. Thanks are also due to the subjects who so kindly submitted to the indignities of the machine.

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DISCRIMINATION AND OPERANT EXTINCTION

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The experiment reported in this paper was performed to test the hypothesis that experi mental extinction of a conditioned operant is facilitated when the stimulus differenc between conditioning and extinction trials is increased. Previously this problem has bee approached in two ways (Bersh, 1951; Dinsmoor, 1951, Notterman; 1951; Skinner, 1938 Bitterman, Feddersen & Tyler, 1953; Winnick, 1951).

First, extinction trials were differentiated from conditioning trials by omitting cue which had been an invariant feature of the reinforcement situation, e.g. the colour of th goal box in a maze-running experiment was changed. And, secondly, cues associated wit the reinforced operant were omitted during extinction trials, e.g. the click of the feedin mechanism in the Skinner box was eliminated. Both experimental techniques are subjec to the same limitation: eliminating or reducing cues previously associated with rewar may induce a general decline in activity without necessarily affecting the rate of extinc tion. To overcome this objection we decided to investigate the part played by discrimina tion in conditioned operant extinction by adding additional cues to the familiar stimulu complex. Thus, whenever the conditioned operant occurred, it was accompanied by novel stimulus. In all other respects the experimental situation was kept identical to th training trials. In the following experiment hungry rats were trained to press a bar i order to secure a small ration of food. Each bar response was accompanied by a flash c light. During extinction trials, the bar response was accompanied by light and a faint sounding buzzer. (A pilot experiment, in which a group was trained with a buzze sounding after each operant, and buzzer-light was presented during extinction trial yielded roughly the same results as are reported here. We preferred using light as th training stimulus because it appeared to facilitate training. We have since found that ligh may be used as a reinforcer of operant responses in its own right. See Hurwitz (1956)

In order to test the prediction that the additional stimulus facilitates operant extinctio it is necessary to rule out the alternative factors: general decline of activity and als specific decline of food-orientated behaviour. In a recently reported study of Melchin (1954), statistically significant differences between experimental groups could not b accounted for satisfactorily, since the functional properties of the stimulus could not b identified. Melching's research design—which has many points of similarity with th present study—is outlined in Table 1.

Groups A and F may be regarded as control groups with respect to B and E. An analys of the data showed that A *v.* B = 1%, A *v.* E = 1%, *A v. F* = 5–10%, E *v.* F = 1% *B v. E* = 5%, *F v. B* = *not significant.*

Table 1

Groups	Frequency of buzzer in training (%)	Frequency of buzzer in extinction (%)	No. of extinction responses
A	100	100	755
B	100	0	574
E	0	100	402
F	0	0	637

The significance values between the various groups indicate that a simple stimulus generalization hypothesis will not account for all the results. Thus, the control groups, A and F, do not appear to be derived from a homogeneous population, and the experimental groups, B and E, reveal a difference amongst themselves depending on the kind of stimulus deviations from training conditions experienced.

In the present study, an attempt is made to obtain a measure of the strength of food-orientated behaviour, as distinct from critical operant responses. The food trough was fitted with a light plastic door. Opening the door closed a circuit and operated a counter. Any changes in food-oriented response vigour under either of the two experimental conditions could thus be detected. Our hypothesis (that the additional stimulus associated with the performance of the operant facilitates its extinction) would be supported if (*a*) the operant frequency under the two experimental conditions differed significantly, (*b*) the frequency of trough responses was constant for both situations.

Subjects. The subjects were twenty-two 3-month-old experimentally naïve male hooded rats drawn from the small inbred colony maintained by the Department of Psychology.

Apparatus. The apparatus consisted of a modified Skinner box, housed in a large, semi-sound-proof chamber. The subject could be observed through glass screens. As in a previously reported study (Hurwitz, 1955) the bar was placed 5 in. to the left of the food trough, thus forcing the subject to shuttle between bar and trough. The bar was mounted 3 in. above floor level. It measured $\frac{1}{2}$ in. square and required a pressure of 2 g. to move it through $\frac{1}{4}$ in. The food trough was shielded by a light opaque door, which had to be nuzzled open before food could be retrieved. The electric circuits were so arranged as to deliver only one pellet of food at a time, i.e. it was impossible for the subject to accumulate a heap of food by repeated pressing on the bar. A small lamp-holder was mounted 7 in. above the bar. When the lamp was on, it gave 3·5 ft.-candles. The buzzer was placed 12 in. away from the box. It emitted a low buzz. On the wall, facing the bar and trough, we fitted a red indicator lamp which provided sufficient illumination for the subject to be observed through the glass screens surrounding the sound chamber. The food delivery machine used in this study has been described elsewhere (Hurwitz, 1955). Its principal merit is its almost noiseless operation.

Records. Records of the subjects' behaviour were obtained in two ways: (*a*) two magnetic counters recorded the number of trough and bar responses; (*b*) a three-channel pen recorder provided data on the serial ordering of responses and time intervals between responses.

Habituation and feeding schedule. Since the subjects were laboratory bred, they were accustomed to frequent handling and proved extremely tame. Approximately 2 weeks before the first training session, each subject was placed into a special feeding-fasting schedule apparatus which automatically made food available once daily for 2 hr. Two days before the first training trial, the subject was transferred from the feeding-fasting apparatus to a special cage, where it remained until the conclusion of the experiment. Each subject was fed once daily, at the time set aside for its experimental trials.

Training. Training extended over 3 days. On day 1, the subject was placed in the Skinner box and allowed to retrieve ten pellets from the food trough. Thereafter, any additional pellets supplied were accompanied by a brief (0·5 sec.) flash of light. This was repeated 20 times. The subject was then left to its own devices. However, whenever it moved towards the bar, E switched on the light. If the bar was actually touched a pellet of food was released into the trough. Ten pellets were delivered in this manner. Thereafter the subject was left in the box until it had secured ten further pellets.

On day 2, the subject was retained in the box until it had obtained thirty pellets by pressing the bar. On day 3, the subject was allowed to secure sixty pellets. If the subject took longer than 40 min. it was withdrawn from the experiment. Only one subject failed to satisfy this criterion of proficiency.

Experimental extinction. On the final day of the experiment each subject was allocated to one of two groups. For group I, the stimulus conditions associated with the critical operant were identical to the training phase: for group II, the stimulus conditions associated with the critical operant were different in that a new stimulus, the faint buzz, was added to the accustomed complex. Each subject was placed in the apparatus and allowed to obtain three pellets of food under the same conditions as marked training trials. Thereafter the experimental extinction session proper commenced. After 1 hr. the subject was removed from the apparatus and the experiment terminated.

Table 2. *Statistical comparison of difference among group I and group II with respect to bar and trough responses*

	Bar responses		Trough responses	
	Mean	S.D.	Mean	S.D.
Group I ($N=11$)	234·9	59·8	200·3	78·1
Group II ($N=11$)	188·5	37·8	229·0	59·0
	$t=2·1$, $p=0·05$		$t=0·82$, p not significant	

Results. Tests of statistical significance were applied to the operant frequency scores and trough response scores for both groups. As $t=2·1$ in the case of bar responses, and $p=0·05$, the two groups may be said to differ significantly from each other. However in the case of food-trough responses, $t=0·82$, i.e. the differences were not significant.

These results accord with the requirements set out in the introduction for supporting the hypothesis that the additional stimulus associated with the operant during extinction trials facilitates extinction.

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THE PSYCHOLOGICAL REFRACTORY PHASE*†

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A reaction-time experiment was performed in which S was presented with two lights and two hand-keys. A flash of the left light served as a stimulus to press the left key with the left hand; the right light to press the right key with the right hand. Trials were given in which both lights flashed; the interval between flashes being randomly selected from nine possible values ranging from 0 to 600 msec., the various intervals occurring with equal probability.

If s_1 and s_2 are the prior and subsequent of a pair of stimuli calling forth responses r_1 and r_2, respectively, it was found: (*a*) that presenting s_2 during the s_1–r_1 interval has no systematic effect on this interval; (*b*) that the s_2–r_2 interval varies markedly as a function of the s_1–s_2 interval, with s_2–r_2 longest for short s_1–s_2.

Starting in the 1880's research on the refractory phase has created a voluminous literature. In the early investigations 'refractory phase' was understood to apply to the difficulty in the rapid re-elicitation of not only nerve and muscle fibres but also of reflexes such as the eye-wink (Dodge, 1913) and the knee-jerk (Dodge, 1926), of voluntary responses (Telford, 1931), of associative (Thorndike, 1927) and of intellectual (Dodge, 1917) processes. In almost all of these cases the interest was in the *magnitude* of responses. More recent analyses of response mechanisms (Craik, 1947, 1948; Fitts, 1951; Hick, 1948; Vince, 1948; Welford, 1952) have made mention of a *psychological* refractory phase; the interest here has been primarily in the *latency* of responses upon rapid restimulation.

Very few experiments have dealt with this latter problem, the principal studies cited being those of Vince (1948) and Telford (1931). Vince's S's were required to follow the motion of a small dot with the point of a pencil. The motion of the dot was discontinuous, consisting of steps to the right and left within a horizontal aperture. Occasionally two steps of opposite direction occurred in quick succession. Reaction-time (*RT*) in this situation was defined to be the interval between the step and the beginning of the appropriate tracking response. It was found that *RT* to the second of a pair of steps was markedly longer than normal if this second step came within 0·5 sec. of the first. (The following abbreviations will be used: s_1 and s_2 are the prior and subsequent of a pair of stimuli calling forth, respectively, responses r_1 and r_2. 'RT_1' and 'RT_2' are used interchangeably with, respectively, 's_1–r_1 interval' and 's_2–r_2 interval'. For the sake of brevity we also say, for example, 'the *RT to a stimulus*' to mean 'the interval between a stimulus and its appropriate response'.)

Telford investigated a situation in which S's were instructed to press a telegraph key on response to an auditory stimulus. The stimuli were presented in a long sequence, the

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† Since the present experiment was performed, and reported in abstract form (Marill, 1953), a similar experiment has been published by Elithorn & Lawrence (1955). The present data may be taken as supplementing those of Elithorn & Lawrence.

interval between any two being randomly chosen from a set of four possible intervals. It was found that the *RT* to a stimulus occurring after a 0·25 sec. interval was abnormally long.

Discussions of the psychological refractory phase have assumed implicitly that the effect had its locus in the central nervous system. In the type of experiment discussed, however, the mechanics of limb movement obscure the facts, if any, of such a central refractoriness. From purely mechanical considerations we can expect it to take longer to initiate a hand-movement to the left when the hand is moving to the right than when the hand is stationary. This might account for part of the effect observed by Vince. Likewise in Telford's experiment we must consider the fact that at short stimulus-intervals the second key-tap of a pair was probably initiated at a time when the hand was just releasing the key after the first tap.

In view of these considerations it was felt to be desirable to perform an experiment on pairs of *RT*'s in which anatomically independent responses could be studied.

Method

Apparatus. (The apparatus described in this section made use of the following commercially available units: Stimulus lights: GE AR2 and GE NE36; oscillator: H-P Frequency Standard 100A; counters: Berkeley 410; pre-determined counter: two Grason-Stadler 461, in cascade.) A dual *RT* apparatus was employed. The *S* was presented with two lights and two hand-keys. In response to a flash of the left light (*L*-light) *S* pressed the left key with his left hand; in response to the *R*-light he pressed the right key with his right hand. Provision was made for measuring both *RT*'s independently and for flashing the two lights in quick succession in either order.

During the session *S* sat alone in a dimly lighted room. His forearms rested on a table in such a way that his hands could comfortably be kept on the keys. At the other end of the table, 5 ft. from his eyes and perpendicular to his line of sight, was mounted a black square, 1 ft. on a side, on which was affixed a small mark which *S* fixated during the trial. Located with centre $\frac{7}{8}$ in. on each side of the fixation mark was a circular opening $\frac{3}{4}$ in. in diameter, from which protruded the front of an electric bulb. The *L*-light was a split-disk-electrode argon bulb giving blue light, and the *R*-light was a similar neon bulb giving red light. Only half the electrode lighted during a flash, presenting *S* with a luminous semicircle. Flashes were produced by pulses approximately 10 msec. in duration and −40 V. in amplitude, superimposed on a −40 V. d.c. level.

During the session *S* wore earphones by means of which white noise could be presented. This served the dual purpose of signalling to *S* that a trial was in progress and of masking disturbing sounds from the laboratory.

The timers used to measure *RT* were constructed as follows: The output of a 10 kcyc. oscillator was fed through an on-off electronic gate into a high-speed counter. The gate opened as the stimulus flashed, letting the signal into the counter, and closed as *S* made his response, shutting off the input to the counter. The number of cycles of the 10 kcyc. signal occurring between the stimulus and the response was thereby displayed on the counter. Dividing this number by ten, we obtained the *RT* to the nearest millisecond.

The interval between flashes was generated by a predetermined counter connected through a gate to our oscillator. The gate opened simultaneously with the first stimulus

After a predetermined number of cycles had passed into the predetermined counter, this unit generated an output pulse which flashed the other stimulus and started the second timer.

Routing switches were provided for flashing the two lights in either order and for flashing only one light. A separate circuit was also provided for indicating key-taps in the absence of any stimulus.

Subjects. Three male undergraduates at M.I.T., ignorant of the purpose of the experiment, were used as *S*'s.

Procedure. A 6 sec. period during which noise was presented to *S* defines the duration of a trial. During a session 108 trials were given at the rate of two each minute. These were broken up into three runs, between which *S* was allowed approximately 5 min. rest. All *S*'s took part in four training and nine experimental sessions. Each condition occurred the same number of times in all sessions, though in a different random order. The 108 trials of a session were broken down as follows:

(*a*) *Seventy-two binary trials.* In these trials both stimulus lights flashed. To make up such a trial a time interval between the two flashes was selected at random from nine possible intervals, ranging in 75 msec. steps from 0 to 600 msec. The interval between the onset of the noise and the first flash was selected at random from four possible intervals: 2·5, 3, 3·5, and 4 sec. One of the two possible orders of flashes was then also randomly selected.

(*b*) *Nine null trials.* It was desired to elicit as nearly as possible errorless performance from *S*. (An error would be said to occur if a key is tapped during a given trial without being preceded by a flash of its appropriate light or if a light flashes without being followed during the given trial by the appropriate key-tap.) In order to minimize the tendency to make errors, null trials were given. In these trials no light was flashed.

(*c*) *Twenty-seven simple disjunctive trials.* As a further check on errors and also to provide additional data, trials were given in which either the *R*- or *L*-light (but not both) flashed. In these trials the s_1–s_2 interval may be considered infinite, since s_2 never occurred.

In addition to the main experiment, as described above, a small subsidiary experiment was attempted, with the aim of finding the effect on *RT* of warning the subject in advance that a given one of the two lights would flash in the coming trial with a given probability. To make the two experiments comparable, eleven trials from this subsidiary experiment were randomly interspersed with trials of the main experiment during each session. The results from this subsidiary experiment proved inconclusive and will not be reported at this time. Since, in these subsidiary trials, *S* was always forewarned, and thus easily able to distinguish these trials from those of the main experiment, the probability that this addition affected the results of the main experiment is considered negligible.

RESULTS

The results of the experiment are displayed in Fig. 1. The 0-interval condition (two simultaneous stimuli) is plotted in both graphs, since the distinction between s_1 and s_2 cannot meaningfully be made in this situation. The single disjunctive trials are plotted in the left-hand graph over the designation '∞' (see (*c*) above). All points represent medians of samples of 72 scores with the following exceptions: the samples of 0-intervals contain 144 scores each, since we do not here distinguish between RT_1 and RT_2; the

samples of the '∞' condition each contain 243 scores. The confidence-intervals were calculated according to the non-parametric method given by Mood (1950).

From the left-hand part of Fig. 1 we see that presenting s_2 during the s_1–r_1 interval has no appreciable systematic effect on this interval. Once initiated, RT's are not disturbed by the initiation of independent responses. If we consider RT_1 to be in some sense a 'normal' reaction time, then we see, from the right-hand part of Fig. 1, that a refractory

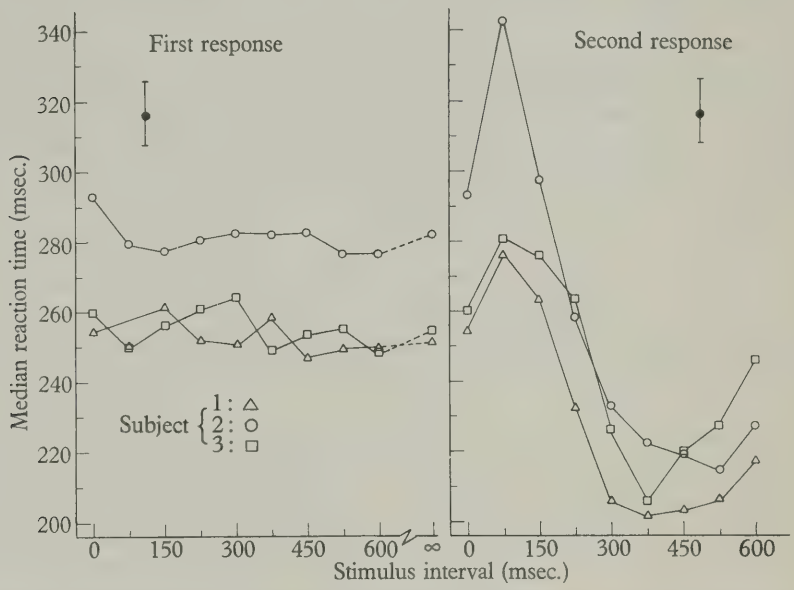

Fig. 1. Median reaction time for all subjects and conditions. The vertical bar in the upper corner of each graph indicates the size of the average 95% confidence interval for points in that graph.

phase is evidenced quite clearly only by subject 2. The comparison between RT_1 and RT_2 is not altogether satisfactory, however, for we must realize that the conditions under which RT_1 is made are not the same as those under which RT_2 is made. It is probably more meaningful to compare the RT_2 only among themselves. We see that there is a very significant change in RT_2 as a function of the stimulus interval. Our original conjecture, that such changes, as observed by Vince and Telford, were due to anatomical considerations, is thus seen to be false, since the responses in the present experiment were anatomically independent. It is quite clear that, in the present experiment, initiating a response during the early part of another reaction time leads to longer RT than initiating this response at a later time.

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THE NORMAL DISTRIBUTION OF INTELLIGENCE: A CRITIQUE

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The dependence of the distribution of test scores on the facility and discriminatory values of the component items is discussed with special reference to the (approximately) normal distribution of quotients from the Binet Scale. A discussion follows as to how this should affect our interpretation of the 'normal distribution of intelligence'.

I. Introduction

The normal distribution occupies a place of central importance in psychological measurement. It is widely accepted that intelligence, along with other cognitive traits, is distributed according to the normal curve. There are many reasons for this. There is, for instance, the layman's misinterpretation of 'normal'. The term is associated with one of its senses in popular usage, and this in turn tends to conceal the supposition that any other distribution of intelligence would be abnormal in the sense of unnatural, a phenomenon to be explained. From this point of view it is unfortunate that 'normal' is generally preferred, as a label, to 'Gaussian', 'Laplacian' or 'probability integral'. To insist on the description of the normal distribution as one in which the frequency of any deviation from the mean is inversely proportional to the antilogarithm of half the square of that deviation (measured in units of standard deviation) might be somewhat repellent to the mathematically unsophisticated. It would serve as a salutary corrective, however, to the naïveté of popular interpretation. In particular, it would permit of no escape from recognizing that the 'normal' of normal distribution is a technical term (and in no way corresponds to the sense of the word in such statements as 'He does not enjoy normal health' or even 'He is of normal height'). It is by no means self-evident that a law of 'normality' underlies the distribution of human traits. Rather it is the existence of such a distribution, not departures from it, which requires explanation in the first place.

There is, also, the argument from analogy with other inherited characteristics. While 'intelligence', as a term in general usage, might not always be identified with innate ability, it is at least regarded as an attribute which is primarily innate. (The general usage would seem to correspond to the geneticist's 'phenotype' if not to his 'genotype'.) Many such attributes, of which height is perhaps the most frequently quoted example, are found to be distributed in a manner which adheres very closely to the normal distribution. 'Intelligence' then—except insofar as its distribution may be affected by environmental factors—can also, it is thought, be expected to exhibit this distribution. While this, too, is an error of laymen rather than psychologists there is a related reason which is more worthy of consideration. This is that the normal distribution of innate ability can be deduced, on certain assumptions, from genetic grounds.

Taking the genetic basis of intelligence as multi-factorial, each factor may be regarded as one of a pair of alternative or allelic genes. Thus in one such pair, Aa, only the allele A would contribute to intelligence, a being neutral. If the total number of pairs is large and

ach allele has always an equal chance of being present in the zygote then the distribu-on of the contributing factors, i.e. the *As*, would be normal, or approximately so. (This llows from the laws of chance in the same way that the successive tossing of a large umber of coins would result in a distribution of 'heads' that is approximately normal.) lthough this is undoubtedly 'an over-simplified genetic mechanism—no account, for stance, is taken of dominance, assortative mating and linkage—Fisher (1918) has shown at a much more general mechanism may be postulated and a normal, or near normal, stribution of the contributing factors still obtained' (personal communication from Sir yril Burt). Fisher chose stature as his illustration of an inherited trait, and Burt (1955) as suggested that the same reasoning would apply to intelligence defined as an innate ility. It is here that the distinction between intelligence so defined and intelligence as e level of cognitive efficiency actually attained becomes important (e.g. Hebb, 1949, . 11) and conclusions which seem justified with respect to the former concept may not e applicable to the latter.* Thus Maxwell (1955), in a discussion of the relative potency heredity and environment on intelligence, contents himself with the modest claim that ese genetical considerations imply 'some justification for the normal distribution of telligence'.

Most important of all, however, is the claim that the normal distribution of intelligence as been established from the results of intelligence tests. In particular, this claim is dvanced with respect to the individual tests of the Binet Scale (e.g. Burt, 1953). It is cognized that the normal distribution of quotients derived from Moray House tests is consequence of the process of standardization (Thomson, 1932; Lawley, 1950). (The stribution of raw scores on these tests is usually rectangular rather than bell-shaped, nd this is afterwards converted to 'normality'.) The distribution of quotients derived om the Binet Scale, on the other hand, is not a statistical artefact, and the empirical ct that these distributions have been found to agree fairly closely with the normal curve often upheld as a convincing vindication of the normal distribution of intelligence. It ill be shown, however, that distributions from both types of tests depend upon the inciples governing the selection of test items. This must inevitably affect our inter-etation of any resulting distribution of quotients.

II. Dependence on item selection

wo statistical criteria determine the selection of test items for Moray House tests (see ilson, 1954). One is an index of item difficulty and the other an index of item discrimi-ation. The former, nowadays termed the *facility value*, is merely the proportion of testees ho pass the item. The latter indicates to what extent those who pass are the successful stees of the test as a whole. Many indices of item discrimination are possible. One is the $_{13}$ value and is determined by the numbers of those passing the item who are in the top nd bottom thirds of the overall order of merit. It is

$$E_{13}=\frac{S_1 \times S_3}{\frac{1}{3}n},$$

here S_1 is the number of top-third successes, where S_3 is the number of bottom-third ccesses and n is the number of testees. Although theoretically E_{13} could vary between

* Burt (1955) however estimates that less than 25 per cent of the variance of intelligence 'as an observable d empirically measurable trait' is due to environmental conditions.

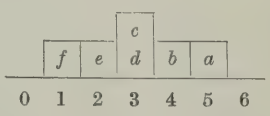
+1 and −1, in practice its value is usually positive. Items whose E_{13} values are less tha 0·5 are usually rejected as unsuitable for selection for a Moray House test (*op. cit.* p. iv If a choice of suitable items is available values near the maximum +1 are favoured. Th facility value of all accepted items must be near 0·5.

Only one statistical criterion determined the selection of test items for the 190 Binet-Simon Scale, i.e. their facility values with respect to the particular age group Binet considered an item suitable for allocation to a certain age level if it could b passed by about two-thirds of the testees of that age. Burt (1921) in his adaptation of th scale, adopted instead the ratio one-half. This ratio is also used in the Stanford an Terman-Merrill Revisions (Terman, 1916, p. 53; Terman & Merrill, 1937, p. 9) and, i fact, still constitutes one of the main criteria for item selection. Another criterion is tha the test items should differentiate between the various age-levels, i.e., the percentage c successful testees should rise steeply with age (Terman & Merrill, 1937). Thus as a child i taken through tests from the various age levels, from the basal age to the level at which a tests are failed, tests with a wide range of facility values are encountered. Item discrim nation was not considered at all in the original construction of the scale. In the Stanfor Revision, however, test items were examined from this point of view (Terman, 191 p. 55) and from data subsequently supplied (Terman *et al.* 1917, p. 133) it would seem tha the E_{13} values of most items are about 0·5, or a little less. Item discrimination is thu lower than is the case with group tests of the Moray House type. It may be shown tha these differences in item selection can account for the contrasting types of frequenc distributions obtained.

Consider a 6-item test undertaken by 6 (equal-sized) groups of testees—*a*, *b*, *c*, *d*, *e* and Suppose that in each of the following illustrations every member of any one grou records the same response to each item. Suppose also that the order of merit of th groups, as given by the complete test, is alphabetical (except where two or more group are equally placed). The E_{13} value of any item is then determined by the presence, in th item successes, of any of the four groups *a* and *b* (for S_1) and *e* and *f* (for S_3). This, togethe with the facility value (*F.v.*), is shown underneath each item. The resulting frequenc distribution is shown alongside.

(i)

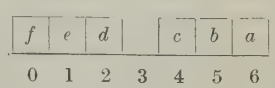
Item no.	1	2	3	4	5	6
Successful groups	*a*	*a*	*a*	*a*	*a*	*b*
	b	*b*	*b*	*c*	*c*	*d*
	c	*d*	*e*	*d*	*e*	*f*
F.v.	½	½	½	½	½	½
E_{13}	1	1	½	½	0	0



Here the facility value of each item is ½, the ideal for allocation to a Moray House test The E_{13} values, however, vary more than would be the case for actually selected items

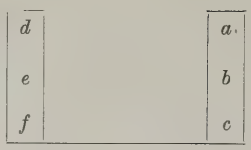
(ii)

Item no.	1	2	3	4	5	6
Successful groups	*a*	*a*	*a*	*a*	*a*	*a*
	b	*b*	*b*	*b*	*b*	*d*
	c	*c*	*c*	*c*	*d*	*e*
F.v.	½	½	½	½	½	½
E_{13}	1	1	1	1	1	0



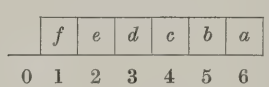
With a more strict selection with respect to the E_{13} values, the central concentration (i) disappears and the tendency towards rectangularity, rather than normality, is eviden

(iii) Item no.	1	2	3	4	5	6
Successful groups	*a*	*a*	*a*	*a*	*a*	*a*
	b	*b*	*b*	*b*	*b*	*b*
	c	*c*	*c*	*c*	*c*	*c*
F.v.	½	½	½	½	½	½
E_{13}	1	1	1	1	1	1



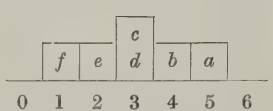
aking an extreme case with each item having an E_{13} value of 1 the departure from rmality is even more pronounced. This is the theoretical limit where all items are rfectly discriminating, each reproducing the result of the entire test. In practice its tainment would be impossible (and indeed undesirable!). The tendency for the distribu-n to be rectangular or U-shaped rather than bell-shaped is clearly illustrated, however, items of low E_{13} values are rejected.

(iv) Item no.	1	2	3	4	5	6
Successful groups	*a*	*a*	*a*	*a*	*a*	*a*
		b	*b*	*b*	*b*	*b*
			c	*c*	*c*	*c*
				d	*d*	*d*
					e	*e*
						f
F.v.	⅙	⅓	½	⅔	⅚	1
E_{13}	½	1	1	1	½	0



ne facility values of the items now vary widely as is the case with tests from the Binet ale. The extremity concentrations of (iii) disappear to yield a rectangular distribution.

(v) Item no.	1	2	3	4	5	6
Successful groups	*b*	*a*	*a*	*a*	*a*	*a*
		c	*b*	*c*	*b*	*b*
			e	*d*	*d*	*c*
					f	*d*
						e
F.v.						
E_{13}	½	½	½	½	½	½



nally in an illustration in which an E_{13} value of ½ is taken throughout and in which the ility value varies between ⅙ and ⅚ the central concentration of (i) returns.

It has thus been illustrated that the frequency distribution of tests is dependent on th the difficulty and discrimination of the component items. In particular: (1) when e facility value of all items is the same, or nearly so, the distribution tends to be tangular rather than bell-shaped, especially if the items are highly discriminating (if the items are highly discriminating the distribution might even be U-shaped); (2) when e facility value varies considerably and the items are moderately discriminating a tribution of central concentration is to be expected.

III. Discussion and conclusions

ny papers have appeared subjecting the suppositions of mental measurement to a ical examination. It is as well that this is so. The attempt to extend the precision of so-called exact sciences to psychology must be scrutinized with care. Indeed some

would maintain that the attempt is futile and that Whitehead's dictum 'to talk sense is t talk in terms of quantities' is inapplicable in the psychological field. Thus Chamber (1943) doubts whether the concept of measurement is applicable at all to psychologica qualities and, more recently, Zangwill (1950, chap. 6) has echoed these views suggestin that nothing psychological is truly measurable. Similarly, Richmond (1953) emphasize the distinction between assessment and appraisal, with the applicability of psychometri techniques confined to the former. However, measurement in psychology need not b identified with measurement in physics* (see Ehrenberg, 1955). Also it need not pre suppose a *ratio* scale, where the existence of both a zero point and units of constant size established. Measurement is also possible with an *interval* scale, e.g. the Fahrenheit scal of temperature, which has no true zero and thus does not permit the comparison of ratio (so that, for example, a temperature of 100° F. does not mean twice as hot as 50° F.). C course the comparison of differences between successive points on the scale would still b permissible. If, however, the units were not constant in size even this comparison woul be vitiated. Such a scale would then be an *ordinal* scale, and would provide a ranking o qualities with no available information concerning possible differences between the rank

If measurement in psychology is regarded as essentially measurement on an ordina scale much of the criticism would be assuaged. Thus while, on such a view, an I.Q. of 12 would denote greater intelligence than an I.Q. of 110, and an I.Q. of 110 greater intelligenc than one of 100, it would be illegitimate to equate the differences between the two pairs o quotients. Intelligence would thus be measurable in the same way as, say, the hardnes of minerals is measurable, and no attempt could be made to compare differences along th scale. Indeed, as Warburton (1955) points out, the applied psychologist would be conten with no greater claim. He is interested primarily in classifying people and is concerne with rank order rather than absolutes. Not all psychologists, however, are content wit this and it is thus claimed that intelligence quotients should be interpreted as measur ments on an interval scale (e.g. Banks & Burt, 1953, p. 161), so that differences betwee quotients may be legitimately compared. A more ambitious claim still is that with scaling technique such as Thurstone's (1925), intelligence measurement may be inte preted as measurement on a ratio scale. The basis of these interpretations is the norma distribution of intelligence. Quotients are interpreted as measurements on an interv scale only when their distribution for a complete year group is normal. Thurstone method of formulating a ratio scale is based on the assumption that this is so for successiv year groups. A more recent attempt by Peel (1949) to construct a scale with constant an additive units is also dependent on this assumption.

The approximately normal distribution of Binet quotients is, as has been stated, a empirical fact. The normal distribution of quotients from Moray House tests, on th other hand, is imposed on the obtained scores during the process of standardization. could be argued that this imposition is more than a statistical artefact in that it enabl measurements by these tests, which have been constructed via the statistical criteria item selection described, to conform with those of the Binet tests. However, results fro

* Thus the physicist Campbell has maintained that the *operation* of measurement is essentially the physic act of placing end to end (Campbell, N. R., *Physics: The Elements* (1920), chap. 10). This excludes a comparab operation in psychology. The *concept* of measurement, on the other hand, may be considered that of compari two or more classes (of objects, individuals, etc.) with respect to a common abstracted quality—and this wou be applicable in psychology no less than in physics.

he Binet Scale are also dependent on item selection. Its bell-shaped distribution of uotients, no less than the roughly rectangular distribution of Moray House tests, may be egarded as a consequence of the facility and E_{13} values of the component items. It would herefore be imprudent to regard the near normal distribution of Binet quotients as erification of a brute fact concerning the distribution of intelligence. In much the same ay as the existence of an absolute zero of temperature is not so much an experimental act about thermal phenomena but, rather, a conceptual matter—a consequence of the ay in which degrees of warmth are put into relation with a number series (see Toulmin, 953, pp. 129–133)—so, too, the distribution of test quotients (and in particular of Binet uotients) is a consequence of the way in which the test has been constructed. This need ot, of course, vitiate the standardization of test quotients to a normal distribution, nor ven their subsequent interpretation as measurements on an interval scale—provided it is ecognized that such measurements are dependent upon the kind of measuring instrument mployed.

The paper commenced with a critical appraisal of some of the reasons for accepting the ormal distribution of intelligence as a fundamental fact of psychology. One further eason will be adduced in conclusion. The distribution agrees with common-sense notions s to how native ability is distributed. We all recognize that the number of geniuses is elatively small. So, too, is the number of mental defectives. Most persons are average, or bout average, in ability. The normal distribution of intelligence fits in with this. Of ourse what is really necessitated from common sense is not the normal distribution as uch, but a humped or bell-shaped distribution of which the normal distribution is but an nstance. Why then should it be the normal distribution which is taken as the best epresentation of fact? The answer, perhaps, is that owing to the extreme usefulness of he distribution in the statistical theory of sampling so many of its properties have been nvestigated. As I was once told by a colleague, a specialist in test construction, it is onvenient to have intelligence follow the normal curve; we know so much about it.

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A ONE-WAY WINDOW

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The wire mesh type of screen described by Gesell suffers from the disadvantage that absolute silence is called for on the part of any observers. Furthermore, it requires a big difference in illumination on the two sides of the screen, as does dark blue glass.

Glass, mirrored in strips, is expensive, and again requires a big difference in illumination. Pinneau (1951) has described a way of making a partial mirror, which differs from an ordinary mirror in having a thinner layer of silver, and therefore permits the penetration of sufficient light for one-way viewing. However, this is troublesome to make, and expensive to obtain commercially.

The screen described is very easy to make, and has proved to be eminently satisfactory. It consists of a sheet of fine-gauge copper-wire mesh (30 to the inch), sandwiched between ordinary plate glass on the front surface (i.e. the testing room side), and dark blue glass, of the type used in ambulances, behind (i.e. the observation room side). The layers of the 'sandwich' are fixed together at the edges by transparent cellophane, which is entirely hidden within the groove of the window support.

Panels of this type can be inserted in the wall between an observation room and a testing room. From the testing room the appearance is of copper coloured panels, which children take to be part of the wall decoration.

As with the Gesell screen, the shiny surface of the wire mesh reflects a good deal of light and produces a diffuse dazzle on the front surface. The amount of light coming from the observation room is diminished by the blue glass. From the observation room side there is clear vision of the testing room, presumably because the blue glass, in absorbing much of the light, prevents dazzle from this surface of the wire mesh.

The window has proved to be satisfactory, even when the level of illumination is the same in the two rooms, though, of course, clarity is increased by heightened illumination in the testing room. Little sound penetrates the screen.

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CONFLICT AND CHOICE TIME

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Four variables have been suggested as determinants of degree of conflict: (1) the number, (2) the nearness to equality in strength, (3) the absolute strength, and (4) the degree of incompatibility, of competing response tendencies. The first two, but not the others, have figured in investigations relating reaction time to information theory. Three experiments were carried out to test the effects of all these variables on choice time. Two types of choice RT were recorded: first, the forced-choice RT (i.e. the traditional choice RT), for which one stimulus was presented at a time, each stimulus having its appropriate response; and second, the free-choice RT, for which two or more stimuli were presented together, S having to respond to any one of them.

The principal findings were as follows. (1) Both types of choice RT were longer when the number of alternative responses was increased. (2) Free-choice RT's were invariably longer than forced-choice RT's, confirming the influence of equality of strength. (3) Forced-choice RT's were inversely related to stimulus intensity, which was regarded as a factor influencing the absolute strength of response tendencies. Free-choice RT's were shortest at the intermediate of the three intensity levels studied and significantly longer at the highest and lowest levels. The proportion by which free-choice RT's exceeded forced-choice RT's increased with stimulus intensity. (4) Degree of physiological incompatibility did not affect free-choice RT's, although physiologically incompatible responses produced longer forced-choice RT's. (5) There were no significant differences between the RT's of S's with high and low neuroticism scores.

The expediency of continuing to include the degree of physiological incompatibility among the determinants of degree of conflict is called in question by the results. Otherwise, choice time, apart from the time taken up by eye movements, etc., which may be shorter with more intense stimuli, appears to increase with degree of conflict. The results are discussed in the light of various theoretical models, derived from Hull's principle of behavioural oscillation, Miller's theory of conflict, information theory, and Hebb's theory of cortical disruption.

I. Introduction

In the early stages of psychology, a great deal of time has necessarily had to be spent on artificially simple situations, in which an experimenter's attention can be focused on one response at a time. But more intricate situations, such as face both human beings and animals in their natural environments, raise a mass of new problems relating to interaction between simultaneously aroused response tendencies. One form this interaction can take is *conflict* (or *competition*), which has already proved to be important in such varied areas as abnormal psychology (Freud, 1929; Pavlov, 1927), perception (Bruner & Postman, 1949), emotion (Hebb, 1949), remembering (Gibson, 1940), and curiosity and thinking (Berlyne, 1954*a*, *b*). The possibility of introducing a variable representing *degree* or *severity of conflict* would therefore seem to be worth exploring. It has, in fact, been suggested (Brown & Farber, 1951; Berlyne, 1954*a*) that such a variable might profitably be regarded as an increasing function of four antecedent variables, namely, (1) the number of competing response tendencies, (2) their absolute strengths, (3) the degree to which their strengths approach equality, and (4) their degree of incompatibility or antagonism.

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One effect that has often been attributed to conflict is a *lengthening of reaction time or ecision time.* It has been reported to occur when children have to choose between npleasant rather than pleasant drinks (Barker, 1942) and when there is a conflict etween what is expected and what is perceived (Desai, 1939; Wilcocks, 1928). A long ssociation time is widely recognized as an indication that a word arouses emotional onflict (Jung, 1918). Choice reaction times are known to increase with similarity between ne alternative stimuli, whether in some physical characteristic (Woodworth, 1938, p. 333–4) or in affective value (Shipley, Coffin & Hadsell, 1945), and Dember (1954) has roposed a method of inferring 'stimulus distance' from decision time. Discrimination f similar stimuli may be taken to involve conflict, on the grounds that each stimulus ends, by generalization, to evoke the responses appropriate to the other stimuli as well s that appropriate to itself.

The theory of conflict links up in two ways with psychological applications of information heory. First, the information theorist's expression for 'uncertainty' or 'entropy', $-\sum_i p_i \log_2 p_i$, resembles degree of conflict in two respects, if we bear in mind the common cceptance of probability as a measure of response strength: it increases with the number f alternatives, and it reaches a maximum when their probabilities are equal. Secondly, hoice reaction time has been shown, in certain conditions, to be a linear function of ncertainty' and, in particular, to increase as the alternatives increase in number and s they approach equiprobability (Crossman, 1953; Hick, 1952; Hyman, 1953; Klemmer Muller, 1953). If we reinterpret 'number of alternative stimuli' to mean 'number of ompeting response tendencies, including those due to generalization', and 'equiprobability f alternative stimuli' to imply 'equal strength of competing response tendencies', we an see that reaction time has been shown to depend on two of the variables held to nderlie degree of conflict. The experiments to be reported in this article were intended o investigate the relations between reaction time and all four suggested determinants f degree of conflict, including the two which have so far not been handled by information heorists. It was hoped that such an investigation might help both to integrate appli- ations of information theory with the main body of psychology and to explore the sefulness of reaction time as a convenient measure of conflict, e.g. for the study of hought processes.

The experiments enabled the traditional choice reaction time (which we may call *'orced-choice RT'*) to be compared with what we may call *'free-choice RT'*. In the orced-choice situation, only one stimulus is presented at any one time, and only the esponse associated with that particular stimulus is permitted. Response tendencies ssociated with the other stimuli may also occur by generalization, so that conflict between hem and the correct response may explain why choice RT's are longer than simple RT's. ut the correct response tendency will be considerably stronger than the others, so that relatively slight degree of conflict will be entailed. In the free-choice situation, two more stimuli are presented together. Each has its corresponding response, and S is ft free to respond to any one of them. Two or more response tendencies of about equal rength will thus arise at once, and, since no more than one can be overtly expressed, greater degree of conflict should result than with forced-choice reactions. We can predict, herefore, that free-choice RT's will exceed forced-choice RT's. It may be worth noting hat Baker & Elliott (1948) have found this to be so in the field of verbal behaviour,

since their experiment showed free-association latencies to be greater than controlled association latencies. The free-choice situation can be seen to typify a large class of everyday dilemmas, in which a decision has to be made between alternative courses of action or as we say, several aspects of the environment 'compete for attention' (Berlyne, 1950, 1951*a*, *b*, 1957).

Our forced-choice situation, like most of the situations handled by psychological information theory or by psychophysics, demands what Platt (1956) calls a *polar decision*. Each of a set of alternative environmental events has its corresponding 'correct' response so that the organism, in order to select its response, has merely to absorb enough information to identify the event. Platt points out that higher animals are often faced with the rather different problem of *equivocal decisions*, imposed on them when one response is not clearly favoured over others, because information, e.g. about remote consequences, is insufficient. Equivocal decisions are generally more time-consuming than polar decisions, and the effects of indecision may be biologically harmful both by wasting time and by generating 'anxiety'. The indecision may be resolved by converting an equivocal decision into a polar one, e.g. by some superstitious procedure, or by resorting to techniques which provide new information (Berlyne, 1954*a*). Our free choices represent yet a third sort of decision, not uncommon in everyday life, which we may call the *arbitrary decision*. The threat of indecision is here greater still, as one response is quite as good as another and receipt of further information cannot help.

Of the two variables whose effects on both kinds of choice RT were to be studied, the *absolute strength* (reaction potential) of response tendencies could be varied by varying the intensity of the stimuli evoking them (Hull, 1949; Logan, 1954). As for the *degree of incompatibility* between responses, a pair of movements that could be performed together —forward movements with both hands—was compared with a pair of movements that could not—forward and backward movements with the right hand. Both conflict theory and information theory have so far confined themselves to cases of complete incompatibility between responses. There is, however, evidence (Konorski, 1948, pp. 120 ff.) that certain responses, if aroused simultaneously, will weaken each other without complete mutual exclusion. It should be pointed out, of course, that any two actions which are *physiologically* capable of simultaneous performance acquire a *learned* incompatibility as a result of instructions not to perform both. Our experiments were therefore comparing a pair of alternative responses having learned incompatibility with another pair having both learned and innate incompatibility.

II. Experiment I

Apparatus comprised three units. First, a *stimulus unit*, comprising a screen with four 1½ in. square apertures in a horizontal line at about eye-level. The apertures were covered with white paper, and small circular spots of light could be made to appear in the middle of any of them. A Palmer Timing Unit caused these visual stimuli to appear for 5 sec. with intervals of 5 sec. between them. E, sitting behind the screen, used push-buttons during the intervals to determine the colours and positions of the stimuli that were to appear next. S sat about 6 ft. from the board, and the room was dimly lit through chinks between black-out curtains and the edges of the windows.

Secondly, *two response units*. One, the C (or 'physiologically compatible') unit, consisted of two switches, each of which could be pushed forward only, mounted on a board. The other, the I (or 'physiologically incompatible') unit, consisted of one switch which could be pushed backward or forward mounted on a different board.

Thirdly, *a Chronotron Millisecond Meter*, set to record RT's in units of 10 msec.

Subjects were twenty male junior-secondary-school pupils, all aged 12 years.

Design. Two sequences of 102 stimulus patterns were drawn up with the help of a random-number ıble. Each comprised forty-one *red* and forty-one *green* stimuli (for forced-choice RT's) and, mingled ith them, twenty patterns composed of *stimuli of both colours* (for free-choice RT's). Three, four or ve forced choices separated every two successive free choices. Colours, positions and combinations of ositions of stimuli all appeared equally often in a random order. One sequence, the '*single*' sequence, sed *one* spot of light for each forced choice and *two* spots, one of each colour, for free choices. The other, ıe '*double*' sequence, used *two* neighbouring spots of the same colour for forced choices and *four* spots, ıcluding two adjacent spots of each colour, for free choices.

Both red and green spots had luminances of approximately 2 millilamberts. The 'double' sequence ıay be assumed to have aroused stronger response tendencies, whether one regards an increase in ıtensity as equivalent to an increase in intensity, on the grounds that both mean an addition to the mount of energy impinging on the retina, or one regards the two neighbouring spots as two stimuli voking the same response, in which case the 'principle of behavioural summation' (Hull, 1943, p. 213; 951, pp. 64–5) implies that the response strength would exceed that produced by one stimulus alone.

Every S had two sessions, 24 hr. apart, of which one was with the C response unit and one with the unit, one with the 'single' stimulus sequence and one with the 'double' stimulus sequence. The S's ere divided into four groups of five, who had the experimental conditions in the following combinations nd orders respectively: C single, I double; I double, C single; C double, I single; I single, C double.

Procedure. When the I response unit was used, S grasped the switch between the thumb and forefinger f his right hand. He was told to push the switch forward as soon as he saw a green light and backward s soon as he saw a red light. The switch was not to be returned to the neutral position until the light ent off. When lights of both colours appeared, either response, but not both, was to be performed. Vith the C response unit, the task was similar, except that both hands were to be used; the right-hand vitch was to be pushed forward for a green light and the left-hand switch forward for a red light. A ew forced choices were given for practice at the start of each session.

Results. For each session with each S, mean RT's for forced choices with green stimuli, orced choices with red stimuli and free choices were calculated. The data are exhibited n Table 1.

Table 1. *Experiment I. Mean reaction times in milliseconds*

	Forced choices			Free choices
	Green	Red	All	
Overall mean	1034	1002	1018	1270
Between S's S.D.	—	—	*199*	*323*
Double stimuli	1000	989	994	1323
Single stimuli	1069	1015	1042	1216
I response unit	1072	1055	1063	1270
C response unit	992	948	973	1269
First session	1043	975	1009	1265
Second session	1026	1028	1027	1274

The prediction that free-choice RT's would be longer than forced-choice RT's was nvestigated with Wilcoxon's T-test for paired replicates (Mosteller & Bush, 1954). The free-choice mean was higher than the forced-choice mean for 19 out of 20 S's, so hat $T=1$ and $p<0{\cdot}01$.

Split-plot analyses of variance (Cochran & Cox, 1950) were carried out on forced-choice nd free-choice means separately. These analyses showed that the 'double' stimulus equence produced *shorter forced choice* ($F=4{\cdot}77$, d.f. $=1, 17$, $p<0{\cdot}05$), but *longer free-hoice* RT's ($F=11{\cdot}92$, d.f. $=1, 17$, $p<0{\cdot}01$) than the 'single' stimulus sequence. The ormer finding is, no doubt, related to the well-known fact that more intense stimuli roduce shorter simple RT's (Hull, 1949), while the latter is in line with our expectation

that an increase in the absolute strengths of competing response tendencies will increas conflict. It should be noted that the free-choice RT's for double stimuli are not onl greater in absolute value than those for single stimuli. They also represent a greate percentage-wise increase over the corresponding forced-choice RT's, the free-choice RT' being 33·1 % longer than the forced-choice RT's for 'double' stimuli and 16·7 % longe for 'single' stimuli.

A comparison of RT's with the two response units showed that the physiologicall incompatible pair of responses (i.e those required by the I unit) produced significantl longer forced-choice RT's ($F = 15{\cdot}36$, d.f. = 1, 17, $p < 0{\cdot}01$), but there was no differenc in the case of free-choice RT's ($F = 0+$). Since the two different response units neces sitated different responses to red but identical responses to green stimuli, an examinatio of forced-choice RT's to green only constitutes the fairest test of the degree-of-incom patibility variable. This, once again, shows the I response unit to have produced longe forced-choice RT's ($F = 10{\cdot}77$, d.f. = 1, 17, $p < 0{\cdot}01$).

There were two other interesting results. One was the significant interaction betwee sessions and colours of stimuli for forced choices ($F = 7{\cdot}68$, d.f. = 1, 17, $p < 0{\cdot}05$). RT's t red stimuli were shorter in the first session, but they lost this advantage in the secon session. This was, no doubt, due to the fact that different responses to red were demande in the two sessions using different response units. In the second session, therefore, ther would be three competing responses evoked by red stimuli: the correct response, th generalized response appropriate to green, and the interfering response learned in th first session. That the RT's increased as a result is a further confirmation of the import ance of the number of competing response tendencies variable. Secondly, the mea forced-choice RT for each S and the difference between his mean free-choice RT and hi mean forced-choice RT were taken, and the product-moment correlation was computed This came to $+0{\cdot}63$, $t = 3{\cdot}47$, d.f. = 18, $p < 0{\cdot}01$. This suggests that an investigation o relations between the two types of choice RT and personality factors may be wort while. A S's forced-choice RT and the extent to which this is exceeded by his free-choic RT may both reflect some characteristic reaction to conflict.

III. Experiment II

A second experiment was undertaken for two purposes. First, it was felt that the degree of-incompatibility factor deserved another test. In Exp. I, different response units wer used for physiologically compatible and incompatible pairs of responses. This meant tha S's might possibly have changed their postures for the two conditions, which might hav affected their RT's, although it would be difficult on this hypothesis to explain wh free-choice RT's were not influenced in the same way. Secondly, it was thought desirabl to see what happened to free-choice RT's when stimuli were fainter than any used i Exp. I.

Apparatus was the same as in Exp. I, apart from the following modifications. (1) The *stimulus un* was timed to present stimuli for 4 sec. at 4 sec. intervals. White and red spots of light were used. F half of the S's the two left-hand holes and for the remainder the two right-hand holes were covere with six sheets of ordinary white typing paper, which made the spots appearing there about as fai as they could be without the colours becoming hard to distinguish. The spots appearing at the tw uncovered holes remained at the order of intensity that was used for the single stimulus sequence Exp. I. The approximate luminances in millilamberts were 6 for the bright white spots, 2 for the brig

d spots, 0·20 for the dim white spots, and 0·06 for the dim red spots. (2) One *response unit* was used. consisted of two switches mounted on a board, both of which could be moved backward or forward. *Subjects* were ten boys and six girls, all aged 10 years and attending a primary school.

Design and Procedure. Two stimulus sequences were drawn up, each composed of sixteen presentations a *red* spot of light, sixteen of a *white* spot and sixteen presentations of *a red and a white spot together*, ndomly interspersed. All four positions for the forced-choice stimuli were used equally often in a ndom order, as were the right-hand and the left-hand pairs of positions for free choices. Half of the rced choices and half of the free choices were thus with bright stimuli and the remainder with dim imuli.

Each S had two sessions of forty-eight responses, one with each stimulus sequence, in immediate ccession. He was instructed to grasp both switches throughout. One session, the first for half of the s and the second for the remainder, was with condition I (physiologically incompatible responses), . the right-hand switch was to be moved forward in response to a white light and backward in response a red light. The other session was with condition C (physiologically compatible responses), i.e. the right-nd switch was to be moved forward in response to a white light and the left-hand switch was to be oved forward in response to a red light. When lights of both colours were presented together, either sponse was to be performed, as with the free choices of Exp. I.

Results. Mean RT's for the various conditions are shown in Table 2.

Table 2. *Experiment II. Mean reaction times in milliseconds*

	Forced choices			Free choices
	White	Red	All	
Overall mean	1208	1190	1199	1428
Between S's S.D.	—	—	*229*	*326*
Bright stimuli	1099	1110	1105	1377
Dim stimuli	1316	1270	1293	1478
I responses	1232	1240	1235	1440
C responses	1184	1140	1162	1415
First session	1202	1188	1195	1454
Second session	1214	1192	1203	1401

A single analysis of variance was carried out on the *total* reaction times for the various onditions. The free-choice totals were doubled to make them comparable with the orced-choice totals, and they were then significantly higher than the latter at the ·001 level ($F = 22{\cdot}62$, d.f. = 1, 15). The difference between I responses and C responses as not significant ($F = 2{\cdot}60$, d.f. = 1, 14). But when forced-choice RT's to white stimuli nly (which evoked identical responses in both conditions) were analysed separately, ' came to 3·53, and $p < 0{\cdot}05$ by a one-tailed test. We have therefore obtained some onfirmation of the conclusion, supported by Exp. I, that physiological incompatibility etween responses lengthens forced-choice RT's. But once again no evidence was found or a similar effect on free-choice RT's. The interaction between I or C responses and orced or free choices is insignificant.

It will be seen from Table 2 that both types of RT were longer with dimmer stimuli, nd, in fact, F for the intensity variable came to 47·91, which, with 1 and 42 d.f., exceeds he 0·001 significance level. But the interaction between forced or free choices and right or dim stimuli produced an F of 4·47, which, with 1 and 42 d.f., is significant at he 0·05 level, showing that the intensity variable had a smaller effect on free-choice RT's. t first sight, the results of Exp. II seem to contradict those of Exp. I, since, in Exp. II, he dimmer stimuli produced the longer free-choice RT's. But if we examine the percent-ge by which free-choice RT's exceeded forced-choice RT's we find the same trend as efore. In Exp. I the percentage was 33·1 for the more intense stimuli and 16·7 for the

less intense. The brighter stimuli of Exp. II (which approximate the less inten stimuli of Exp. I) produce a percentage of 24·6 (the difference being explicable by t change in S's and conditions), and the dimmer stimuli a percentage of 14·3.

It seems likely that slower eye movements were at least partly responsible for t lengthening of both types of choice RT with fainter stimuli. Receptor orientations a possibly other sensory or neural processes necessary for dealing with a stimulus m take a certain minimum time, so that only part of a choice RT may reflect the degree of co flict between the switch-pressing responses. The relative slowness of eye movements, et with fainter stimuli may itself be attributable to conflict. Receptor orientations can regarded as forced-choice reactions, and, the fainter the spots of light, the more nea equal the tendencies to fixate the four apertures.

The correlation between forced-choice RT's and the excess of free-choice over force choice RT's for individual S's came to +0·24, which is not significant. But the analy of variance showed a significant interaction between S's and forced or free choic ($F = 5{\cdot}31$, d.f. = 15, 42, $p < 0{\cdot}01$).

IV. Experiment III

Exp. III also had two purposes. The first was to verify that an increase in the number alternative responses would lengthen free-choice RT's as well as forced-choice RT The second was to find out whether Eysenck's neuroticism (P) factor (1947) would affe performance. It was thought that individuals with higher neuroticism might show higher proportional increase of free-choice over forced-choice RT's.

Apparatus was as in the two previous experiments, except that only green spots appeared at the t end positions and only red spots at the two middle positions. The same luminances as in Exp. I w used, and a vertical line of luminous paint divided the two right-hand from the two left-hand apertur The room was almost completely dark. The *response unit* was that used in Exp. II.

Subjects were twenty-four undergraduates, selected as follows: The Maudsley Personality Invento was given to thirty-four men and forty women attending a first-year psychology lecture. The scores the Neuroticism Scale and the Lie Scale were computed. Eight high- and low-neuroticism S's who h recently participated in another experiment were omitted, as were two low-neuroticism men with score of more than 10 out of 20 on the Lie Scale and one low-neuroticism man who failed to respond a request to take part. Apart from these, the twenty-four S's selected consisted of the six men and t six women with the highest and the six men and the six women with the lowest neuroticism scor The ranges for these four groups were (out of 24) 16·5–21·5, 18·5–23, 2·5–6·5 and 7–9 respectively.

Design and procedure. Every S went through a *four-alternative* sequence, in which all four apertu of the stimulus unit were used, and a *two-alternative* sequence, in which only the two apertures on o side of the central dividing line were used. Three S's in each group had the two-alternative sequence, w the two right-hand positions, before the four-alternative sequence. The remaining three had the fo alternative sequence first, and in their case the two-alternative sequence used the two left-hand positio

Both sequences comprised thirty-six stimulus patterns. The two-alternative sequence included twel presentations of a *green* spot in one position, twelve of a *red* spot in the other position and twelve *both together*. The four-alternative sequence included twelve presentations of a *green* spot, the two e positions being used equally often, twelve of a *red* spot, the two middle positions being used equa often, and twelve of *two green and two red spots simultaneously*. The first four patterns of the fo alternative sequence consisted of one presentation of each single stimulus, but otherwise the sequen were randomized.

S was instructed to respond with the key that was on the same side as the spot of light, so that o one key was used when there were two alternatives, but both keys when there were four alternati The key was to be pressed forward in response to a green stimulus and backward in response to a one. There were thus as many alternative responses as there were alternative stimuli. When two or f spots lit up together, S was to respond to any one of the stimuli and ignore the rest.

Results. The mean forced-choice and free-choice RT's in both two-alternative and ur-alternative conditions were calculated for each S. These means are displayed in able 3. They were subjected to a logarithmic transformation before undergoing analysis variance, because the standard deviations increased steeply with the means.

The analysis showed that free-choice RT's were significantly longer than forced-choice T's ($F = 22{\cdot}64$, d.f. = 1, 48, $p < 0{\cdot}001$) and four-alternative RT's were significantly nger than two-alternative RT's ($F = 276{\cdot}78$, d.f. = 1, 48, $p < 0{\cdot}001$). The interaction etween these two variables did not approach significance ($F < 1$). We can therefore nclude that both forced- and free-choice RT's increase when the number of alternative sponses rises but, in view of the logarithmic transformation, that the proportion by hich the latter exceed the former is not significantly different whether there are two or ur alternatives. This proportion was actually 10·1% with two and 10·4% with four ternatives.

None of the comparisons or interactions involving neuroticism or sex reached significance, ith the sole exception of the neuroticism × order interaction ($F = 10{\cdot}32$, d.f. = 1, 16, $< 0{\cdot}01$). The two-alternative to four-alternative sequence produced lower RT's than e other sequence in the case of the high-neuroticism S's but higher RT's in the case of e low-neuroticism S's.

Table 3. *Experiment III. Mean reaction time in milliseconds*

	Two alternatives		Four alternatives	
	Forced choices	Free choices	Forced choices	Free choices
All S's	585	644	807	891
Between S's S.D.	*97*	*141*	*183*	*207*
High-neuroticism S's	589	633	836	886
Low-neuroticism S's	581	654	779	896

V. Discussion

ne theoretical considerations with which this paper began have performed one of the incipal functions of theory: they have directed our attention to variables whose portance was subsequently confirmed by experimental data. It now remains to discuss e significance of the findings further.

They are in general accord with the conclusion that choice time, in so far as it exceeds e minimum time for registration of a stimulus, increases with three of the proposed terminants of degree of conflict, viz. the absolute strength, the nearness to equality d the number of competing response tendencies. The physiological compatibility-of-sponses variable, however, turned out to affect forced-choice but not free-choice RT's. ne most likely explanation for this last fact seems to be as follows. Let us suppose that S is in a forced-choice situation and begins to make the wrong response. If the correct sponse consists of moving the same hand in a different direction, it cannot be initiated fore the wrong response has been cancelled. But if the correct response consists of a ovement with the other hand, it can be initiated earlier. The quicker correction of rors may well explain the shorter average RT's with the latter condition. With free-oice RT's, on the other hand, either response is acceptable, and so the question of rrection does not arise.

At all events, it seems doubtful whether the degree of *physiological* incompatibility

should continue to be included among the determinants of degree of conflict. There seem however, to be cases of *acquired* incompatibility, e.g. between symbolic responses par ticipating in thought processes (Berlyne, 1954*b*), where the extent of the incompatibility is of importance. Miller (1944, p 458) describes two relevant types of acquired conflict In the first type, 'the individual learns to respond with anxiety to the pattern produced by simultaneous stimulation from both responses'. The degree of incompatibility can, in such instances, be identified with the strength of the anxiety-mediating habit. In the second type, 'the subject learns to react directly to the stimuli produced by each o (the responses) with a response incompatible with the other', i.e. if the two origina responses are R_1 and R_2, the proprioceptive and other stimuli resulting from R_1 come t evoke a response R_{-2}, which is incompatible with R_2, and stimuli resulting from R_2 com to evoke R_{-1}, which is incompatible with R_1. When this happens, the degree of incom patibility can be identified with the ratio of the strength of R_{-1} to that of R_1 and th ratio of the strength R_{-2} to that of R_2 and thus reduced to the equality-of-strengt variable.

We can now turn to various theoretical models, treating phenomena closely akin t those that are our concern here, and see whether they can account for the relation between the determinants of degree of conflict and reaction time. It may be enlightenin in this connexion to consider not why free-choice RT's are relatively long but why S' ever come to react at all. What must occur before one of the competing reactions ca prevail over the other? The different models suggest different answers.

(1) One possible answer is that some relatively improbable combination of events mus come about before the conflict can be resolved, and that at least part of the mean R consists of the time that must elapse before chance can be expected to produce thi combination.

According to Hull's principle of behavioural oscillation (1943), response tendencies hav 'momentary effective reaction potentials ($_S\bar{\dot{E}}_R$)', which fluctuate independently wit approximately normal distributions. A response will not be performed unless its $_S\bar{\dot{E}}_R$ i higher than the 'reaction threshold'. There are three models that can be based on thes assumptions.

(*a*) Hull's own statement about competing response tendencies appears as Postulate 1 in *Principles of Behaviour*. It is that only the response with the highest $_S\bar{\dot{E}}_R$ will occu But when it occurs, there is no reason why it should not have the same latency as if i had been evoked alone, since latency (Postulate 13) depends only on $_S\bar{\dot{E}}_R$ (or according t later versions of the theory (1951, 1952) on $_SE_R$). Moreover, it should take no longer fo one of two strong response tendencies than for one of two weak ones to have a higher $_S\bar{\dot{E}}_R$

(*b*) An alternative model is provided by the plausible additional assumption that a overt response will not occur until the fluctuations have brought one response tendenc above the reaction threshold at a time when the other response tendencies are below i

Let us assume that the probability of the $_S\bar{\dot{E}}_R$ of a certain response exceeding th threshold in a particular unit of time is p. The portion of the mean simple RT whic depends on behavioural oscillation would then be $1/p$ units. Let us now suppose tha we have two such responses competing. The probability that either will be above-threshol while the other is not will be $2p(1-p)$, and the corresponding portion of the mean fre choice RT will be the reciprocal of this expression. Now the value of $\frac{1}{2p(1-p)}$ reaches

inimum when $p = 0{\cdot}5$. The free-choice RT will be longer with stronger response tendencies, ecause both tendencies will be above the threshold for most of the time, and it will be nger with weaker response tendencies, because both tendencies will be below the ıreshold for most of the time. The curvilinear relationship between free-choice RT and imulus intensity that is suggested by Exps. I and II fits this implication of the model ery well, but unfortunately there is another implication that does not agree with the ata. This is that, when $p < 0{\cdot}5$, $\frac{1}{p} > \frac{1}{2p(1-p)}$, which means that the simple RT will be nger than the free-choice RT. Since forced-choice RT's are known to exceed simple T's, it follows *a fortiori* that forced-choice RT's will exceed free-choice RT's when esponse tendencies are weak enough to bring the latter above their minimum. This onclusion is not borne out by the results of Exp. II, and so the model is not tenable. This model might possibly be applicable to situations in which there are very weak abits. For example, 'name the capital of Great Britain' might conceivably produce a uicker response than 'name any city beginning with L', whereas 'name the capital of araguay' might produce a slower response than 'name any South American capital'.)

(*c*) Another possibility is that the ${}_S\dot{\bar{E}}_R$ of one response must exceed those of all ompeting responses by a certain minimum quantity k before overt action can take place. he probability of this happening will be least, and consequently the average number f time units before it happens will be greatest, when the response tendencies are equal strength. The probability will also diminish, and the reaction time will therefore go up, hen the number of response tendencies is increased. In order to predict that the part f the free-choice RT dependent on behavioural oscillation will increase with the absolute alues of the ${}_S\dot{\bar{E}}_R$'s, we have to add the postulate, analogous to the Weber–Fechner law, at k is larger with stronger response tendencies. If the intensity of one stimulus has exceed that of another by a certain proportion before an organism reacts to the fference, it is not unreasonable to suppose that random fluctuations must make the ${}_S\dot{\bar{E}}_R$ of one response exceed those of its competitors by a certain proportion before it can clipse them. The quantity k would thus bear the same relation to the differential sensory ıreshold as Hull's reaction threshold (${}_SL_R$) bears to the absolute sensory threshold. he fluctuations that give one of two equal response tendencies this temporary advantage ould be analogous to those that cause one of two equal stimuli to be judged more tense than the other in a psychophysical experiment. This model seems, so far, to be ıe most promising of those derived from the concept of behavioural oscillation.

All these probabilistic models imply some variation in the responses a S will select in ıccessive exposures to a free-choice situation. Various factors may make one of the ternative responses more frequent than another, but the models could scarcely stand ɔ if a S always made the same response. In order to verify that this does not happen, supplementary experiment was carried out. The S's were six boys from the same school ıd age group as the S's for Exp. I. All of them received the '*single*' sequence from xp. I with 3 sec. on to 3 sec. off timing. Half performed the C responses and the other alf the I responses. The mean number of responses to red stimuli, in the red-green free-ıoice situations, was 8·5 out of 20, the range being 3–12.

(2) According to Miller's (1944) classification, our free-choice situation produces an ɔproach-approach conflict. A choice should be made in favour of one alternative as on as random variation of behaviour has inclined the organism towards one of the

alternatives. Miller's theory does not imply that this would take longer to occur when stronger approach tendencies are competing. An assumption might, however, be added to the effect that a larger inclination in one direction, which would presumably take a longer time to occur, is needed to tip the scales when the conflicting approach gradients are higher. This would produce a model closely related to model 1(*c*).

(3) According to Hick's information-theory model for the forced-choice situation (1952, 1954), the organism absorbs information at a more or less constant rate and is ready to act when its uncertainty has fallen to a certain minimum, comparable to a statistical significance level. This model does not seem to be applicable to the free-choice situation. In particular, it does not explain why free-choice RT's exceed forced-choice RT's or why the former vary with the absolute strength of response tendencies.

The resolution of free-choice conflicts, as Platt (1956) points out, means a loss of information both by 'equivocation' (since several combinations of stimuli can result in the same response) and by 'noise' (since one stimulus pattern can give rise to any of several responses) (Shannon & Weaver, 1949, chap. II). The S has to discard some information, as we are all constantly doing when we concentrate our attention on one part of the environment and make no use of the rest of the information that is being showered on our sensory surfaces. He also has to make a decision which creates information. These processes appear to take time.

(4) A model of yet another sort is suggested by Hebb's (1949) theory of emotion. The 'disruption of cortical organization' which underlies 'emotional disturbance' may, Hebb argues, result from conflict between incompatible phase sequences. The disruption 'may vary in extent *and* in duration', leading 'when it has more than momentary duration', to 'some inco-ordination of effectors', as Hebb's studies of rage and fear, as well as the work of Desai (1939) and Luria (1932), illustrate. The hypothesis might be ventured that the duration of cortical disruption increases with the variables we have cited as determinants of degree of conflict and that responses are delayed until disruption has subsided.

This model derived from Hebb's theory may well be capable of integration with one of the others. It needs, in fact, to be supplemented, if only to explain how one response comes to be chosen when action is no longer blocked. Emotional and motivational factors may not be out of place, for example, in information-theory analyses. Hick (1952) found that forced-choice RT's went down when S's were allowed to make some errors. If S's refrain from responding until their uncertainty with respect to the identity of the stimuli is low enough, the amount of uncertainty they are prepared to tolerate may plausibly depend on how much they are afraid of making a wrong decision. Similarly, our S's had markedly higher RT's, for both forced and free choices, at the beginnings of sequences than later, and we may surmise that a higher level of anxiety on approaching an unfamiliar task was responsible. On the other hand, we failed to find any difference between S's at opposite ends of the distribution for 'neuroticism', a trait associated with proneness to emotional disturbance.

Cerebral processes are thought by Hebb to be so susceptible to disruption because each depends on delicate synchronization between two or more antecedent processes. The probabilistic models discussed under (1) likewise make overt action depend on an appropriate relation obtaining between two or more variables at a particular moment. It may thus be possible to merge model 4 with model 1(*c*), by postulating that cortical

sruption will last until the superiority of one ${}_S\dot{E}_R$ over its rivals reaches the 'differential action-threshold'.

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ERSONALITY FACTORS AND THE RATE OF CONDITIONING

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Sixty normal male students were conditioned, using the eyeblink reflex. It was found (1) that those students who were introverted conditioned considerably better than those who were extraverted, and (2) that there was no correlation between conditioning and neuroticism. This experiment was designed primarily to provide support for an introversion-extraversion theory of conditioning as opposed to Spence's drive theory. The implications of the experiment are discussed and related to other experiments designed to show that Hull's Seventh Postulate, as used by Spence, leads to erroneous deductions. It would seem that conditioning is independent of drive level where the drive is a secondary factor in the conditioning situation.

I. Introduction

ull (1943, p. 240 ff.) offers evidence to suggest that behaviour is a function of two rincipal classes of variable—learning and motivational. These two variables are supposed combine multiplicatively according to the formula, response $=f({}_SE_R)=f(D\times {}_SH_R)$, here ${}_SE_R$, D and ${}_SH_R$ represent three theoretical constructs, namely, reaction potential, ive strength and habit strength (Postulate no. 7). According to Hull ${}_SH_R$ is a dependent ypothetical learning variable, and its value can be calculated (Hull, 1943, p. 178 ff.); practice this would seem to be impossible since it is neither clear what his four critical nstructs (k, j, u and i) are, nor how they should be determined. In addition, Hull ates that there are some other independent variables which may affect the value of H_R and which cannot at present be defined. D is also a hypothetical construct and is pposed to represent the total drive strength operating in the organism at a given oment, thus in any specific situation the value of D is assumed to be determined by oth relevant needs and the total aggregate strength of existing irrelevant needs,

Spence and his colleagues (e.g. Spence & Farber, 1953; Spence & Taylor, 1951; Taylor, 951) have adapted Hull's model, in particular the above equation, to make predictions bout eyelid conditioning. They argue that the total effective drive strength (D) is in art a function of the level of internal anxiety or emotionality of the subject; consequently bjects with a greater degree of anxiety would possess more drive and therefore form nditioned responses better than subjects with a lesser degree of anxiety. They have ed Taylor's Manifest Anxiety Scale (Taylor, 1953) as their measure of anxiety, and aim that, in general, this prediction has been confirmed. (This scale and its relationship conditioning is discussed elsewhere in this paper.)

An entirely different theory has been developed by Eysenck (1955*a*, *b*) on the basis of rtain Pavlovian concepts and hypotheses. (Pavlov's original observation (1927, pp. 397 f.) as to the effect that neurasthenics possessed an exaggeration of the excitatory process id hysterics an exaggeration of the inhibitory process.) Retaining the notion of excitation and inhibition potentials as determining ${}_SE_R$, Eysenck has postulated individual fferences in the development of these two potentials and linked these differences with rsonality development according to the following postulate: 'Individuals in whom active inhibition is generated quickly, in whom strong reactive inhibitions are

generated, and in whom reactive inhibition is dissipated slowly are thereby predisposed to develop extraverted patterns of behaviour and to develop hysterico-psychopathic disorders in cases of neurotic breakdown; conversely, individuals in whom reactive inhibition has developed slowly, in whom weak reactive inhibitions are generated and in whom reactive inhibition is dissipated quickly, are thereby predisposed to develop introverted patterns of behaviour and to develop dysthymic disorders in cases of neurotic breakdown' (Eysenck, 1955*a*, p. 35).

The well-established finding (e.g. Spence and his colleagues; Welch and his colleagues, 1947*a*, *b*) that anxious subjects condition better than non-anxious may be explained either in terms of Spence's theory of superior drive level or in terms of Eysenck's theory of inhibition-excitation and extraversion-introversion. (Eysenck (1952, 1953) has established that anxious subjects are more introverted and more neurotic than normal subjects.) A crucial experiment would be one for which each theory would predict different results. One such experiment is that in which drive is manipulated, while introversion-extraversion remains unaltered; Spence's theory would predict a change in conditionability, Eysenck's theory would predict no change. To increase drive a group of normal male subjects were deprived of food, drink and tobacco for 24 hr., while another similar group were allowed to eat, drink and smoke in their usual manner. It was found that both groups conditioned equally well (Franks, 1957).

Another crucial experiment would be one which examined the conditionability of three groups, namely, normals, introverted neurotics (anxiety states), and extraverted neurotics (hysterics and psychopaths). Spence's theory would presumably predict that both groups of neurotics would condition better than the normal group (the assumption being that high neuroticism, or high emotionality, may be associated with high drive). Eysenck's theory would predict that the introverted neurotics would condition best, the extraverted neurotic worst and that the normals—being as a group neither introverted nor extraverted—would be intermediate in their ease and strength of conditioning. Such an experiment was carried out by the present writer (Franks, 1956*a*). It was found, using both the eyeblink reflex and the PGR, that patients diagnosed as anxiety states conditioned readily, and patients diagnosed as hysterics or psychopaths conditioned poorly, the normals being in between. Several other findings are of relevance, in particular that both neurotic groups had high scores on Taylor's anxiety scale, whereas the normal group had a low score on this scale. Furthermore, like other workers (Bitterman & Holtzmann, 1952; Hilgard, Jones & Kaplan, 1951), the writer found no significant correlation between conditioning and Taylor's scale. It was also found that conditionability was unrelated to neuroticism as measured by the Maudsley Medical Questionnaire or by Guilford's *D* and *C* scales (1940), but was closely related to introversion-extraversion as measured by Guilford's rhathymia scale, *R* (1940). (The evidence for the Maudsley Medical Questionnaire and Guilford's *D* and *C* scales being measures of general neuroticism and Guilford's rhathymia scale being a measure of extraversion is presented by Eysenck (1952, 1953) together with a general discussion of these scales. Guilford's scales are also discussed elsewhere in the present article.) Spence's hypothesis, therefore, appears to gain no support from the experimental findings detailed above, and it seems reasonable to conclude that when Hull's equation, *as used by Spence*, is applied to conditioning situations where the drive is irrelevant, then conditioning is dependent not upon the drive component but only upon the excitation component, i.e. upon ${}_{S}H_{R}$.

Two objections to these findings may be raised. First, whereas Spence and his colleagues orked mainly with normal student volunteers, the writer used mainly hospitalized eurotic patients. Secondly, since it was impossible to avoid knowing or guessing into hich of the two categories each patient fell it could be possible that attitudinal factors ere systematically influencing the results. Therefore it would seem desirable to carry ut a similar study on a group of normal student volunteers about whom nothing relevant as known and, as a further precaution, have a different experimenter administer the uestionnaires.

II. PLAN OF PRESENT EXPERIMENT

Subjects. Sixty paid undergraduate male volunteers were used ranging in age from 8 to 26 years. All subjects were given a meal allowance and travelling expenses. None f them was studying psychology as a main subject, and all of them were naïve with espect to experimental methods in psychology.

Conditioning apparatus. The conditioning apparatus and laboratory has been described detail elsewhere (Franks, 1956*a*). Briefly, the unconditioned stimulus was an air puff f 500 msec. duration and pressure 65 mm. of mercury, delivered from a 2·5 mm, internal iameter polythene tube situated approximately 2 cm. from the right eye. The con-itioned stimulus was a pure tone, delivered to both ears through a pair of high-quality added earphones; its frequency was 1100 cycles per sec. and its duration 800 msec., an intensity of 65 db. above the frequency threshold and so arranged that the air uff began 350 msec. after the start of the tone. All time intervals were accurately ontrolled by means of a small photoelectric cell (Franks & Withers, 1955) mounted pon the same lens as supported the air-puff delivery tube. The air was supplied from electronically operated gas cylinder, and the recording milliammeter was so arranged iat eyelid movements and the occurrences of the various stimuli all appeared on the same ecord. The experiment took place in a sound-proof laboratory (Franks, 1955) so arranged iat the apparatus and experimenter were in one half of the room and the subjects being onditioned in the other half, the two parts of the room being separated by a sliding irtain.

Personality questionnaire. All subjects were given a new questionnaire, the Maudsley ersonality Inventory, recently developed by Eysenck (1957) from the questionnaires of ie Guilfords (1934, 1936, 1939*a*, *b*) and from the Maudsley Medical Questionnaire. The eed for a new questionnaire arose because the Guilford scales, although they can be sed to measure the two orthogonal dimensions of introversion-extraversion and neuroti-sm, have been observed to possess certain limitations. First, it would seem that in the erivation of these scales little attention has been paid to sex differences; working with iese scales suggests that some items in each scale are more predictive of a high score on iat scale in the case of men than for women, and vice versa. Secondly, the claim by ie Guilfords that each of the scales measures a unitary factor is dubious; it would seem iat certain scales are far from unitary and can be resolved into two unrelated com-onents. There is good evidence (Eysenck, 1953) to indicate that when a factor analysis the intercorrelations between the Guilford scales is carried out two clear-cut orthogonal ctors emerge, these two factors being identifiable as neroticism and introversion-traversion respectively, scales *C* (cycloid disposition) and *D* (depression) most charac-rising neuroticism, and scales *R* (rhathymia) and—to a lesser extent—*G* (general activity)

and *A* (ascendancy) characterizing introversion-extraversion. Other studies strongly support these conclusions (Eysenck, 1955*a*, *b*). Thirdly, although the *R* scale has a low correlation with both the *C* and the *D* scales, an item analysis showed that certain individual items in the *R* scale correlated more highly with the total score on the *C* scale or the *D* scale than with the total score on the *R* scale. A similar deficiency was found in some of the *D* and *C* items.

The new scale was constructed in such a way that these limitations were largely obviated. The neuroticism scale (*N*) and the introversion-extraversion scale (*E*) each contain twenty-four items, so that the test is considerably easier to give and to score than the earlier questionnaires. All items in the *N* scale have significant relations with Guilford's *C* and *D* scales, for both men and women, and insignificant relations with Guilford's *R* scale. All items in the *E* scale have significant relations with Guilford's *R* scale, for both men and women, and insignificant relations with Guilford's *D* and *C* scales (Eysenck 1956). Sex differences are small for both *E* and *N* scales and the intercorrelation between the two scales is $-0{\cdot}048$ for a sample of 200 men and 200 women. The split half reliabilities of both scales are high.

III. Procedure

The subject was first given the Maudsley Personality Inventory by an independent experimenter. He was then told that records would be made of how well he 'relaxed under various conditions when in a quiet room' and brought to the conditioning laboratory where he was seated in an armchair, complete with padded head and foot rest. This chair was arranged so that his field of vision was almost completely confined to a small booth immediately in front of him. After the subject was made comfortable, the apparatus was fitted, his hearing threshold taken and the intensity of the tone stimulus adjusted accordingly. Before commencing the conditioning session proper, brief tests were carried out for initial eyeblink sensitivity to sound and for pseudo-conditioning. A technique of partial reinforcement was used, consisting of thirty reinforced trials interspersed with eighteen test trials (called acquisition trials) consisting of only the conditioned stimulus. After the sequence of thirty reinforcement trials and eighteen acquisition test trials had been given, the subject received a further series of ten successive test trials (called extinction trials). The intertrial interval varied from 20 to 30 sec., and the complete session lasted approximately half an hour.

IV. Results and discussion

Of the sixty subjects five had to be rejected for the following reasons: two because of excessive blinking, rendering the records unscorable; one because only irregular unconditioned responses were obtained; one because of an experimental error during testing and one because the questionnaire was incomplete. For the remaining fifty-five subjects the correlations between conditioning and introversion-extraversion (*E*) were $-0{\cdot}46$ for the number of acquisition conditioned responses and $-0{\cdot}34$ for the number of extinction conditioned responses. These correlations are negative because *E* is a measure of extraversion, and the theory requires the prediction that introverts would condition well and extraverts relatively poorly. The correlations are very similar to those reported between eyeblink conditioning and Guilford's *R* scale of extraversion for a group of sixty subjects

consisting mainly of hospitalized neurotics, namely, −0·48 for the acquisition scores and −0·37 for the extinction scores (Franks, 1956*a*). The correlations between conditioning and neuroticism, as measured by the *N* scale of the Maudsley Personality Inventory, were 0·04 for the acquisition measure and 0·15 for the extinction measure. Using a one-tailed test of significance, the two correlations between conditioning and extraversion are highly significant ($p < 0{\cdot}01$), whereas the two correlations between conditioning and neuroticism are insignificant. These results strongly suggest that in normal subjects also conditionability is related to introversion-extraversion and not to neuroticism.

Table 1. *Conditioning and personality scores*

Group		Conditioning		Extraversion	Neuroticism
		Acquisition	Extinction	(*E*)	(*N*)
All 55 subjects:	Mean	7·18	2·57	13·09	8·90
	S.D.	5·12	2·65	4·49	5·65
High extraversion (15 most extraverted):	Mean	3·73	0·60	1840*·	8·27
	S.D.	3·64	1·14		5·57
Low extraversion (15 least extraverted):	Mean	9·87	3·53	7·53†	9·13
	S.D.	4·87	2·75		4·06
High neuroticism (15 most neurotic):	Mean	5·73	2·27	14·47	16·27‡
	S.D.	5·15	2·85	3·86	
Low Neuroticism (15 least neurotic):	Mean	6·00	2·20	13·60	2·60§
	S.D.	4·02	2·16	3·89	

* This is the mean extraversion score of the top fifteen subjects on the extraversion scale, the range of their scores being from 17 to 22.

† This is the mean extraversion score of the bottom fifteen subjects on the extraversion scale, the range of their scores being from 10 to 3.

‡ This is the mean neuroticism scale of the top fifteen subjects on the neuroticism scale, the range of their scores being from 12 to 22.

§ This is the mean neuroticism score of the bottom fifteen subjects on the neuroticism scale, the range of their score being from 5 to 0.

Table 1 presents the mean number of acquisition conditioned responses given by the fifty-five subjects, together with the mean number of extinction conditioned responses. The mean extraversion score (*E*) and the mean neuroticism score (*N*) are also given. Similar data are provided for the fifteen subjects scoring highest on the extraversion scale and the fifteen subjects scoring lowest on this scale, and for the fifteen subjects scoring highest on the neuroticism scale and the fifteen subjects scoring lowest on this scale. Applying *t*-tests to these data produces the following results:

High versus low extraversion. The high extraversion group give significantly fewer acquisition conditioned responses than the low extraversion group ($t = 3{\cdot}79$; $p < 0{\cdot}0005$). Similar results were obtained for the extinction conditioning scores ($t = 3{\cdot}71$, $p < 0{\cdot}0005$). The high and low extraversion groups are very similar in their neuroticism scores and certainly are not significantly different in this respect ($t = 0{\cdot}47$).

High versus low neuroticism. The high and low neuroticism groups are very similar in their ease of conditioning and certainly are not significantly different (acquisition, $t = 0{\cdot}155$; extinction, $t = 0{\cdot}073$). These two groups are also very similar in their extraversion scores ($t = 0{\cdot}59$).

Figs. 1 and 2 present the data of Table 1 in a different manner. In Fig. 1, the total number of conditioned eyeblink responses given by the most extraverted fifteen subjects and by the least extraverted fifteen subjects is plotted for each of the eighteen acquisition

test trials and each of the ten extinction test trials. It is clear from this figure that th fifteen subjects with the lowest extraversion scores condition much better than th fifteen subjects with the highest extraversion scores. (It might be argued, from Fig. 1 that the more introverted group condition better because they start off better. However by test trial no. 7 both groups condition equally readily; from then onwards the mor introverted group condition far better and furthermore are not completely extinguishe after ten successive extinction trials, as are the more extraverted group.) In Fig. 2, th high and low neuroticism groups are compared with respect to their conditionability It is clear that the fifteen most neurotic subjects closely resemble the fifteen least neuroti subjects in their conditioned response behaviour.

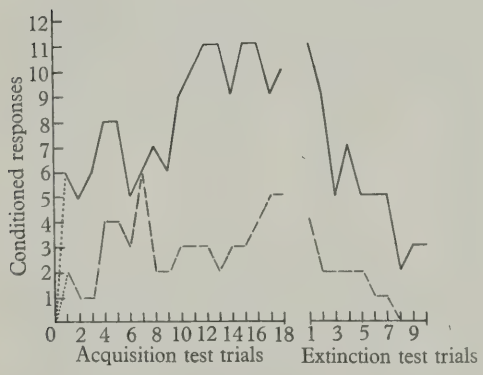

Fig. 1

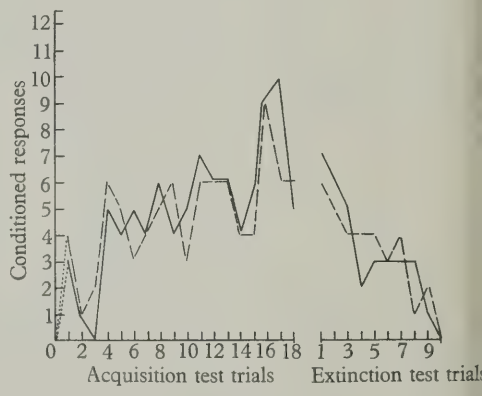

Fig. 2

Fig. 1. The total number of conditioned eyeblink responses given by the fifteen most extraverted subjec (broken line) and by the fifteen least extraverted subjects (solid line).

Fig. 2. The total number of conditioned responses given by the fifteen most neurotic subjects (broken lin and by the fifteen least neurotic subjects (solid line).

From these results it would seem reasonable to conclude that conditionability is closel related to introversion-extraversion, and not at all related to neuroticism, as these tw personality dimensions are measured here. Two conclusions would seem to follow fro the experiments discussed so far. First, that drive does not affect conditioning in situatio where the drive is an irrelevant one. Secondly, that conditioning is unrelated to neu roticism, but closely related to the personality dimension of introversion-extraversion.

The first conclusion requires some elaboration. It is, for example, clear that drive may increase the learning of instrumental responses (e.g. see Munn, 1950, pp. 305 ff. but it would seem difficult to conceive of eyelid conditioning in the present series experiments as primarily and consistently instrumental. It is also clear that unde certain circumstances, drives increase conditioning when the drives are directly relevan to the conditioning stimuli, thus Zener & McCurdy (1939) found that hungry dogs yiel a greater conditioned salivary response than non-hungry dogs, and Lashley (1916) an Winsor (1928) obtained similar results with humans.

The second conclusion is, in part, related to the first, since neuroticism is often regarde as a state of heightened emotionality, and it is not unreasonable to regard emotionalit as a form of drive. The second conclusion is of interest also because many workers— quite independent of any drive theory—have expected neurotics to condition better tha

ormals (e.g. Finesinger, Sutherland & McGuire, 1942; Fink, 1943; Schilder, 1929). urther evidence supporting the relationship between conditioning and introversion-xtraversion is the finding that sodium amytal, a cortical depressant, reduces condition-bility and at the same time increases the degree of extraversion (Franks & Laverty, 955). This finding is in accord with the general formulation that Pavlov's cortical rocesses of excitation and inhibition underlie the personality dimension of introversion-xtraversion.

If conditioning is closely related to introversion, then it would be expected that a ear positive relationship between conditionability and anxiety would emerge, since nxious subjects have been shown to be introverted. However, Spence and his colleagues btained only poor relationships between anxiety and eyelid conditioning—their corre-tions, although positive, were sometimes insignificant and always slight. The explanation ould seem to be (Franks, 1956*b*) that their measure of anxiety, the Taylor Anxiety cale, has an extremely high correlation with the Maudsley Medical Questionnaire, which as been found (Eysenck, 1953) to be an adequate measure of general neuroticism. At the me time, Taylor's scale has a considerably smaller correlation with introversion, this rrelation being sufficient to account for the positive but poor results obtained by pence and his associates. It would seem, then, from the above conclusions and the vidence presented, that Spence's use of Hull's Seventh Postulate leads to erroneous eductions and is, in fact, incorrect in situations where the drive concerned is a secondary ctor in the condition situation; what appear to be the determining factors are individual ifferences in the cortical processes of excitation and inhibition.

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AN EXPERIMENTAL INVESTIGATION OF SEWING-MACHINE SKILL

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An attempt was made to measure the transfer effects which occur when the receptor or effector modes of an element of sewing skill are changed. The pre-transfer task required the operation of an industrial sewing machine for about twenty years. Under these conditions no transfer was detected. It is concluded that this particular skill is dependent on the utilization of the particular receptor and effector modes with which it was originally acquired. It is suggested that transfer experiments are not likely to be fruitful unless learning is controlled and, in the case of motor skills, adequate attention is paid to the way in which the peripheral system is being utilized.

I. Introduction

sensori-motor skill such as that developed by sewing-machine operators is dependent the timing and precision of limb movements. The correct movements must be made th the correct speed, the correct force, in the correct order, and at the correct time. is usually found, however, that as the cycles of activity follow each other none of the ove remain constant as they would if the task was controlled by a machine. Small anges in the texture of the leather, the size and style of the shoe, etc., which alter the der and timing of the movements do not appear to interfere in the least with the eady progression of the activity. The less competent operator is less economical both time and of movement. That is, she takes longer over every part of the operation d makes more frequent changes of grip and work position. The problem then is: what the structure of the skill? Or, to put it another way—what has the skilled operator arned? Or, what particular abilities has she acquired which have not been developed the unskilled operator? The identification of these so-called key elements of the skill an important preliminary step in any investigation of ways to improve operators' iciency, to redesign controls, to train operators or to assess the capacity of new labour. This skill is too complex to be examined as a whole in a controlled situation. The ex- riments described below are concerned solely with foot movements and the relevant ceptor processes. When the treadle is pressed the machine accelerates, the operator sees e work moving under the needle, she hears the machine noise and the receptor organs her feet and leg muscles initiate signals concerning what has been done. The higher e skill of the operator the less she needs to attend to all these signals. An inexperienced erator does not know exactly what the machine will do and she has not acquired the ility to grade the muscular response accurately. So she must take time to assimilate e knowledge of results which becomes available, and to correct errors. A highly skilled erator who has made similar responses so often in the past is almost certain about what ll happen when she makes the particular response so that all the events which follow nvey much less 'information' to her and are therefore more easily and more quickly alt with.

If we accept this as a reasonable assessment of the situation a further problem arises. what internal mechanism does the operator control her foot responses and what cues

does she utilize? If these questions can be answered even approximately then we hav a theoretical base from which to consider ways of reducing the skill required, e.g. b providing better cues or changing the response required (i.e. by changing the controls so that the precision required of the operator is less severe.

It so happens that during a particular sewing skill known as the Derby side operation certain quite specific responses are required of the operator. On the standard Gibso upper there are two short rows of stitching which strengthen each flap containing eyelets A third row at right angles to these along the front edge of the quarter fulfils a simila purpose. These rows are all about eight stitches in length. Thus, in sewing one shoe th operator has, among other tasks, that of making six pulses on the treadle each of whic must produce eight stitches. For a particular shoe model the fixed number of stitche must be reproduced accurately—if the row is too long the appearance of the shoe i impaired. For this response it seems reasonable to suggest that experienced operator develop a central mechanism which provides a response of a given time length and whic is relatively independent of sensory cues. This can be investigated experimentally usin a transfer situation. If such a mechanism exists at a central level then it might be relativel independent of the effector mode of response and should be revealed by positive transfe from one mode to another.

II. Apparatus

A display panel was constructed which consists of twenty-four small neon bulbs equall spaced in a horizontal straight line with a separation of 1 in. between adjacent bulbs The line of bulbs is inset symmetrically in a dull black surface of size 30×12 in. Thes bulbs can be lit in two ways. (*a*) Using a Morse key which can be controlled by the han or the foot. When the key is pressed the end light on the left-hand side is lit. This i followed by the next light and so on until the key is released. The speed at which suc cessive lights come on can be varied by the experimenter through three stages whic give speeds of 2300, 3000 and 3750 lights per minute. When the lights are set to run a the slowest speed and the key is pressed for 0·21 sec., then eight lights will appear o the display. (*b*) Using a sewing machine which can be controlled either by a standar treadle or by a hand lever. As the machine stitches, lights appear on the screen, runnin from left to right, at the same speed as that at which stitches are being made. Th maximum speed of the machine is 2300 stitches per min. A tachograph is attached t the sewing machine, so that when the treadle is pressed a graphical record of the machin speed during the response is obtained.

The experimenter is provided with another Morse key which will zero the light system The subject's Morse key is connected so that having made one response, that is, havin pressed and released the key to produce a certain number of lights, further pressures o the key have no effect until the system has been returned to zero by the experimente

III. Subjects and procedure

Twenty-one female subjects were used. Eleven were from the staff of the Researc Association. None of these had any experience in using an industrial sewing machin The second group of ten were machine operators in a shoe factory, all of whom wer accustomed to producing the kind of response required in this experimental situatio Their average length of experience on sewing machines was over 20 years.

Each subject was given the same tasks in the same order. A vertical red line was ed on the display between the 8th and 9th lights. The subject sat comfortably in front the display (which was roughly at eye level) with one hand on the Morse key. It was plained that when the key was pressed lights would 'run' from left to right across the splay. The subject was to press the key for just long enough for the lights to reach the l line, and a few practice trials were given. When the subject made a response which oduced eight lights she was told that this was correct. When she made responses ich produced either more or less than eight lights she was told that she was wrong d that she must try to produce eight lights at each attempt. When it was apparent that e subject understood the task she was given three experimental trials, each of sixty-ur responses. After each response the lights were returned to the zero position by the perimenter. During the first trial the speed was 2300 lights per minute, during the cond trial 3750 per minute, during the third trial 2300 per minute.

The subject was then asked to sit at the sewing machine. Again the display was about t. in front of her. It was explained that the lights would now effectively work from e sewing-machine treadle and would run at the same speed as that at which the machine s stitching. She was asked to do exactly as she had done in the earlier trials and stop e lights at the red line. She was also asked to make the response as quickly as possible d to try to put on the eight lights in one burst rather than by attempting to stitch ry slowly. After a few practice attempts at this the subject was given the fourth and al trial of sixty-four responses using the machine.

About 8 months after the completion of the first part of the experiment the procedure s repeated using the Morse key controlled by a foot pedal and the machine controlled a hand lever. The same sequence of tasks and, as far as possible, the same subjects re used. Because of staff changes it was necessary to introduce two new subjects into e operator group, but the remaining eight and all the non-operator group were the same those used for the first part of the experiment.

IV. Results

ter each response the experimenter noted the number of the right-hand end light. r this purpose, when eight lights were on the number 0 was noted, when seven or nine hts were on -1 or 1 was noted respectively. Similarly for six lights the number -2 uld be recorded, for ten lights $+2$, and so on. The sixty-four scores of one trial can en be plotted as a distribution which appears, in most cases, to be normal in shape. om this distribution various measures of the skill can be derived. One measure is the mber of correct responses. In most cases the mode and median of the distribution curred at a score of zero. A second possible measure is the standard deviation of the tribution which indicates the consistency or the efficiency of 'grouping' of the 'shots'. third possible measure is the mean of the distribution which reveals any tendency to ershoot or undershoot during the trial. The means of these three measures for each of e Morse key trials are shown in Table 1. Standard deviations of the scores are shown brackets. The distributions of the subject's scores are such that on none of the three res is there any significant difference between operators and non-operators. It will o be noted that there is no learning at this task and no difference in performance tween trials involving the fast and slow light speeds.

Table 1. *Number in sample: ten operators; eleven non-operators*

		1st Trial (slow)	2nd Trial (fast)	3rd Trial (slow)
Number correct	Operators	13·5 (3·0)	13·1 (7·3)	14·2 (5·4)
	Non-operators	16·2 (7·5)	14·5 (3·2)	17·5 (9·4)
Standard deviation	Operators	2·37 (1·01)	2·21 (0·85)	1·74 (0·51)
	Non-operators	1·85 (0·14)	1·89 (0·58)	1·60 (0·54)
Mean	Operators	0·25 (0·79)	0·33 (0·65)	−0·17 (0·33)
	Non-operators	0·28 (0·68)	0·39 (0·65)	0·11 (0·61)

The same scores can be derived from the fourth trial in which the sewing machine wa used. In this case, however, there are two other measures of performance available These can be obtained from the tachograph records. The maximum machine spee during a response can be extracted. From these the mean maximum machine speed an the standard deviation of the maximum machine speeds during a trial can be calculated All these scores and their standard deviations for this trial are shown in Table 2. It wi

Table 2

	4th Trial	
	Operators	Non-operators
Number of subjects	10	11
Number correct	20·4 (4·2)	12·5 (3·5)
Standard deviation	1·29 (0·10)	2·08 (0·71)
Mean	−0·09 (0·35)	0·76 (0·82)
Mean maximum speed obtained*	64·7 (7.9)	58·2 (10·7)
Standard deviation of maximum speeds	9·6 (1·7)	13·2 (4·0)

* The speed units are such that the maximum possible machine speed is 100 units.

be seen that, at this task, the machine operators, as compared with non-operators (*a*) achieve a greater number of correct responses ($t = 4{\cdot}15$, $p < 0{\cdot}001$); (*b*) achieve a mor consistent response ($t = 3{\cdot}13$, $p < 0{\cdot}01$); and (*c*) achieve a response which on average i more correct ($t = 2{\cdot}93$, $p < 0{\cdot}01$). The machine operators also achieve a higher and mor consistent maximum machine speed, but these differences are not significant. Individua differences between members of the group are less in the case of operators. That is, al the operators have about the same score by each measure. Of the non-operators one o two are about the same as the operators, but other non-operators are poor by comparison

The means and standard deviations of the various scores obtained during the thre trials using the foot-controlled Morse key are shown in Table 3. There is no significan difference in any score between operators and non-operators. Comparison with Table suggests that there is little difference between performance with the hand and with th foot.

Table 3. *Number in sample: ten operators; eleven non-operators*

		1st Trial (slow)	2nd Trial (fast)	3rd Trial (slow)
Number correct	Operators	14·0 (6·1)	12·8 (4·2)	17·6 (5·6)
	Non-operators	15·1 (6.8)	9·0 (7·1)	19·3 (6·4)
Standard deviation	Operators	2·07 (0·17)	2·00 (0·66)	1·79 (0·32)
	Non-operators	1·81 (0·58)	2·22 (0·69)	1·51 (0·50)
Mean	Operators	0·36 (0·84)	1·07 (1·32)	−0·33 (0·66)
	Non-operators	1·18 (1·01)	2·37 (1·33)	0·17 (0·52)

The final trial in this experiment involved hand control of the sewing machine. The sults are shown in Table 4. In contrast to Table 2 it is quite clear that in this case there no difference between operators and non-operators.

Table 4

	4th Trial	
	Operators	Non-operators
Number of subjects	10	11
Number correct	16·1 (3·7)	14·9 (3·5)
Standard deviation	1·62 (0·37)	1·85 (0·37)
Mean	−0·05 (0·53)	+0·25 (0·67)
Mean maximum speed obtained	74·7 (1·0)	76·9 (3·5)
Standard deviation of maximum speeds	6·8 (2·0)	8·0 (2·0)

V. Conclusion

difference has been detected between machine operators and non-operators in ability make a precise and consistent short response by: (*a*) pressing the finger on a key; pressing the foot on a key; (*c*) using the hand to control a sewing machine. In ability make a similar response using the feet in control of a sewing machine, machine operators significantly better than non-operators.

VI. Discussion

this experiment is considered as a transfer situation, the pre-transfer task consists about 20 years' experience in producing short runs of stitching within a more general ll. The experiment involves an experimental group (sewing machine operators) with evious training and a control group (non-operators) without previous training. Since th groups were exposed to the same sequence of experimental tasks, it seems feasible neglect the transfer effects between these tasks. Thus there were effectively four post-nsfer tasks. (i) The foot-treadle sewing machine. This involved using the same receptor d effector systems to make isolated responses which result in short runs of stitching able 2). (ii) The finger-controlled Morse key. This involved making isolated responses ng different sensory cues and a different effector mode (Table 1). (iii) The foot-ntrolled Morse key. In this case the effector system is unchanged but the receptor stem is different (Table 3). (iv) The hand-controlled sewing machine. The effector stem is changed but the receptor system is only partially modified (Table 4).

The difference between the groups for (i) suggests that the element under investigation l transfer from a general serial skill to a more isolate situation. This is important ce, as Bartlett (1948) has pointed out, the study of skill by the isolation of particular rts of the general task can be misleading. Confirmation that in this case the isolated sponse will transfer out of the serial skill is necessary before any further conclusions n be drawn.

In the introduction it was suggested that sewing machine-operators might have quired a central mechanism appropriate to this specific response which would function dependently of peripheral mechanisms. From (ii) there is no evidence to support the stence of such a mechanism. It remained possible, at this stage, that the mechanism envisaged might be detected by leaving either the effector or receptor system un-anged. This was attempted in (iii) and (iv), but again there was no evidence of positive

transfer. It seems then that this particular motor skill is very sensitive to changes o the receptor and effector modes.

This approach to the problem of transfer is related to the classical analytical metho first developed by Poffenberger (1915) and Wylie (1919). These and later experimenter have concluded that useful generalizations concerning transfer can be made in terms o response and stimulus similarity. The change of terminology from stimulus/response t receptor/effector emphasizes the importance of considering how the subject acts rathe than what the action achieves. The consideration of transfer in terms of stimulus an response has led to the relative neglect of the specific information channels which ar utilized and the amount of 'coding' of information which is necessary to carry out th task effectively. Thus some experimenters have dealt with situations in which visua symbols are used to evoke a linguistic response (learning of nonsense syllables) whil others have used visual displays which require merely a direct motor response. The sam stimulus/response conceptual framework is applied. Yet in the former case the informatio is quite strictly channelled in and out with the highly practised visual and vocal system so that any transfer effects which occur are almost certainly central in origin. In the cas of motor skills where there is a great variety of sensory cues available it seems unlikel that the situation can be understood unless attention is paid to the particular ways i which the peripheral systems are being utilized.

To return to the experiments described above, it will be noted that many years hav passed since the operators actually learned their pre-transfer task. It seems reasonabl to assume that in this case we are dealing with the transfer of skill as such and not wit the mixture of this and transfer of 'learning how to learn' which has confused the result in most experiments where all the transfer tasks are learnt in the laboratory within short period. These experiments can be regarded as further evidence to support th hypothesis that the large transfer effects which are normally obtained in the laborator are largely dependent on the transfer of learning technique rather than the utilizatio of particular stimulus/response mechanisms. This may be of some practical importance since the present rate of technical change in industry is increasing the necessity fo workers to change their skills. If the above hypothesis is valid they will not gain muc assistance from positive transfer, but at least the old skill will not impede the learnin of the new one.

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A NOTE ON THE ROLE OF 'GUIDANCE' IN LEARNING

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The traditional experiments on 'forced correct responses' as a method of guidance have indicated that its facilitative effect on learning is rather small. It is suggested that this outcome of the experiments was mainly due to the fact that the subjects during the guided trials got little or no information as to the location of the choice points or the possible choices at these. An experiment is described showing that when such information is provided, 'forced correct responses' may be a very efficient form of guidance. The importance of information about possible errors for the functional discrimination of the correct responses is briefly discussed.

I. Introduction

number of studies have been concerned with the efficiency of various kinds of guidance perceptual-motor learning. With guidance the learner does not discover the solution a problem or the most efficient way of performing a task independently, errors being inimized by guiding the learner—wholly or partly—through the correct responses. several experiments errors have been prevented altogether during a part of the learning riod, the subject being forced to make only correct responses.

These experiments have mainly been carried out using stylus mazes. The typical ocedure has been to guide the subject along the correct path from start to goal during me stage of learning and then to discontinue the guidance, leaving the subject to actice for himself until the criterion is reached. The main modes of 'forcing' the bject's responses have been (*a*) mechanical guidance, by blocking the entrances to e blind alleys (Koch, 1923); (*b*) manual guidance, the experimenter guiding manually e stylus along the correct path (Ludgate, 1923; Melcher, 1934; Wang, 1925; Waters, 30); and (*c*) verbal guidance, the experimenter telling the subject what moves to make ile traversing the maze (Wang, 1925). The results from the various experiments are fairly good agreement, showing that guidance in most cases is favourable to learning, relative effectiveness decreasing with an increasing amount of guidance, and decreasing e earlier it is inserted in the learning process (cf. Carr, 1930). The best results were tained with a small amount of guidance during the very early trials. However, even der favourable conditions the facilitation due to guidance was found to be rather slight, d in no case could it to any major extent replace actually doing the task to be learned 'trial and error'. For example, even when subjects were given forty or eighty initial ided trials on a maze which was learned on the average in 37·5 trials without any idance, they still required (on the average) more than seventeen trials to reach the terion when allowed to trace freely (Waters, 1930).

These results are still widely quoted without further comment in text-book chapters the economy of learning (e.g. McGeoch & Irion, 1952; Wolfle, 1951). In view of this d of the importance of the issue for various training procedures, it may be of some erest to point out that an important limitation characterized the experimental ocedure in all the studies cited above; during the guided trials the subjects were ven little or no information as to the location of the choice points and the possible

choices at these. The movements required to trace a maze are generally quite easy t perform once the maze is adequately structured perceptually. Usually the choice poin in a maze serve as 'reference points' for the organization of the pattern to be learne When clear-cut information about these points is omitted, it seems likely that 'force correct responses serve to facilitate the general orientation toward the goal as well a the building up of a general 'picture' of the true path, but a more detailed organizatio becomes difficult in the absence of such information.

Most experimenters in the field of maze learning agree that subjects preferably lea most types of mazes by a 'verbal' method, and that this generally is by far the mo efficient way of dealing with a maze problem (cf. von Wright, 1955). Taken by itsel the correct path is mostly a rather complex affair and it is comparatively difficult t learn as a purely 'motor' sequence. But even those subjects who learn by a motor metho do not normally learn to perform the sequence of movements along the correct path a a serial skill which may be correctly executed in the absence of appropriate extern guiding stimuli. This is indicated, *inter alia*, by the fact that when subjects who hav learned a complex maze are asked to draw the correct patterns on a blank sheet of pap on reaching the criterion, the reproductions made by subjects using a motor method learning are on the average even more inaccurate than those drawn by subjects using verbal method. Irrespective of the learning method employed, the absence of a fixed s of reference points will thus presumably make the task difficult to structure. The subje may, of course, try to learn all the characteristics of the path (many of which are irrelevan to the subsequent correct performance of the task), or he may act on the expectatio that only some particular aspects of the path (e.g. the direction of the turns) are of im portance, while disregarding other aspects such as the distances between the turn However, if information as to the choice points and the possible choices is supplied, th task is much simplified in that the subject knows what characteristics of the path a the relevant ones, and can concentrate on learning only these. In other words, the rang of possibly relevant information is considerably reduced. In addition to this, such in formation presumably increases the discriminability of the correct responses, as the are 'reinforced' as being *the correct ones out of sets of specified alternatives.*

It is thus suggested that 'forced' correct performance of a maze task is a fairly in efficient form of guidance as long as the task cannot be adequately structured perceptual (or verbally), that the conditions in the traditional studies of guidance were rath unfavourable to this 'structuring', and that giving the subjects information about th choice points during the guided trials will considerably increase the efficacy of guidanc Some experimental data obtained by the writer are relevant to these suggestions.

II. Experimental apparatus and procedure

A detailed description of the apparatus and general procedure used has been reporte elsewhere (von Wright, 1955). Briefly, patterns consisting of twelve diamond-shape units separated by short straightways were drawn on strips of paper and repeated presented to the subjects on a revolving drum. The subjects traced the moving patte with a stylus through an aperture in a screen placed in front of the drum. The apertu was 2 cm. wide, permitting only a part of a unit to be seen at a time. All the choic were made from the same spatial point, and (as the speed of presentation was constan

equal intervals. The time interval between any two successive choices was 1·75 sec., ie total time per trial being 21·8 sec. Between successive trials there was an interval of) sec. The split-half reliability of the task, errors on odd *v.* even trials (guided trials omitted), xceeded in each case 0·92 (uncorrected). The correct pattern was *LRRLRLLLRRLR*, nd the learning criterion was two successive errorless trials.

Sixty undergraduates, unfamiliar with learning experiments and with the problem udied, served as subjects. They were randomly divided into three groups. In group A ontrol) the subjects learned throughout by the anticipation method. When a choice ad been made, information about its correctness was given by two red lines drawn across ie wrong path. Both experimental groups received *four initial guided trials*, after which iey immediately continued to learn the task by the anticipation method under conditions ientical to those of group A. In group B (guidance 'with information') the subjects

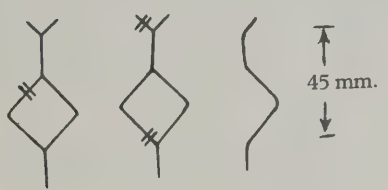

Fig. 1. A part of the pattern as presented under each of the three experimental conditions.

ere given full information during the guided trials about the location of the choice oints and the possible choices at these. This was achieved by using the same pattern iat was presented to the control group, but drawing the red lines indicating wrong ioice close to the choice points where they could be seen prior to the moment of (overt) ioice. In group C (guidance 'without information') the subjects were presented, during ie guided trials, with the correct path only. The differences between the three conditions in be seen from Fig. 1, in which is shown a portion of the pattern as presented to each the groups during the four first trials. After the fourth trial, the pattern 'A' was esented in a similar manner to all subjects. Non-correction procedure was employed roughout. The instructions were the same for all groups, apart from an additional ause in those given to groups B and C, in which the subjects were told that they (in oup B) would be forced to make only correct choices during the initial trials, or (in oup C) would be presented with the correct path only during these trials. No infor- ation about the shape of the pattern was given in the instructions.

Results

The mean trial scores and error scores for each group are presented in Table 1. For oups B and C the trial scores are given separately for all trials and for the unguided ies only. From the error score of the control group (A) the errors made during the four st trials have been excluded in order to make the three groups comparable, as no rors could be made during the guided trials in groups B and C. (The mean total error ore for group A was 98·6.) The time scores are not presented, as they—the task being ced—are strictly parallel to the trial scores.

In all scores group B (guidance 'with information') was very much superior to th other groups, while C (guidance 'without information') was to a less extent—but stil significantly—better than the control group. Forced correct responses 'with information could in fact wholly replace any trial and error learning; out of ten subjects who wer given twelve guided trials under condition B, eight were subsequently able to perfor the task by the anticipation method (condition A) without making a single error. Th results are thus in agreement with the prediction.

Table 1. *Mean trial scores and error scores*

	Trials			Errors (after fourth trial)	
	Total		Unguided		
Groups	*M*	σ	*M*	*M*	σ
A	24·25	7·61	—	76·30	37·17
B	10·45	5·22	6·45	15·80	15·38
C	18·80	4·13	14·80	52·15	26·92
Differences					
A – B t =	6·52			6·56	
p =	0·0001			0·0001	
A – C t =	2·74			2·29	
p =	0·01			0·05	
B – C t =	5·47			5·11	
p =	0·0001			0·0001	

III. Discussion

Under the conditions of the present experiment forced correct responses facilitate learning much more when information about the choice points and the possible choice at these was provided than when it was omitted. In the latter case the subjects onl got 'undifferentiated' information about the correct responses, and had difficulties i identifying these responses later when they appeared in the context of a set of fairl similar alternatives. Presenting the correct alternatives as contrasted with the wron ones served to increase their subsequent identifiability, and provided the subjects wit clear-cut points of reference.

Although the experiments discussed have been concerned only with maze learnin the principles involved seem to have a more general applicability. A number of task are similar to the maze task in consisting of part-activities which in themselves a fairly easy, the main difficulty being to discriminate functionally between correct an incorrect alternatives at successive points of the performance. Here information abou alternative 'wrong' cues (or possible wrong responses) while the subject is guided perform the task correctly serves to indicate which are the dimensions of variation which the subject should learn to respond differentially (or where the response shou be a particular one within a set of similar responses and not any one of this set). Lookin at the problem more broadly, such information may, in a variety of situations, serve facilitate the discrimination of 'cue' stimuli. Thus it was found by the writer in expe ments on concept formation in children and the complex visual discrimination learni in adults, that the more extensive the information about possible 'relevant' errors, t more easily were the correct cues discriminated from any *new* alternative wrong on However, the available data do not permit any extensive generalizations, and there a

ome obvious limitations to the usefulness of 'forced correct responses with information', n the present sense, as a method of teaching, in particular when applied to the teaching f motor skills (cf. Welford, 1951).

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ATTENTION TO CHANGE, CONDITIONED INHIBITION ($_SI_R$) AND STIMULUS SATIATION

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Berlyne (1951) reported an experiment on attention to change. Human S's were firs subjected to a habituation phase, in which they had to make key-pressing responses t a series of homogeneous visual stimuli. They then underwent the experimental phase in which they had to make choices between responding to stimuli of the same sort a before and responding to others of a different shape or colour; they showed a tendenc to respond more frequently to the *novel* than to the *familiar* stimuli.

A further experiment has since been performed with a view to investigating some o the questions left unanswered by the previous study. In the experimental phase of th latter, the novel stimuli always appeared at the same place for a given S. It is possible therefore, that he kept watch on that place in a way which accounts for the greate probability of responding to novel stimuli. One purpose of the present experiment wa to show whether attention to change occurs when novel and familiar stimuli may appea at the same places, in which case a preference for responding to novel stimuli must b due to processes (which may include receptor adjustments) coming into play after th stimuli have been presented.

Another object of the experiment was to test predictions from two possible explanation of the phenomenon. Berlyne (1951) hypothesized that Hull's 'conditioned inhibitio ($_SI_R$)' was responsible. This is a learned and hence permanent inhibition affecting particular *S*–*R* bond. On this hypothesis, the attention-to-change effect should be jus as strong if 24 hr. elapse between habituation and experimental phases. An alternativ concept that might be invoked is 'stimulus satiation', introduced by Glanzer (1953) a a factor assumed to underlie the rat's spontaneous alternation in the T-maze and takin the form of a reduced tendency to make any response to a recently exposed stimulus A similar temporary stimulus-specific inhibition (to use Hinde's (1954) terminology figures in Broadbent's theory (1953). Attention to change, if it is due to such a facto should be just as strong if the responses performed in the habituation phase differ fro those performed in the experimental phase.

Procedure

The S sat facing a vertical wooden board, about 4 ft. 6 in. away, with four $2\frac{1}{2}$ in. squar apertures, numbered 1–4, in a horizontal line at about eye level. A circular spot c red or white light, *c.* $\frac{3}{4}$ in. in diameter, could appear in the middle of each aperture A Palmer timing unit made such stimuli appear for 5 sec. with 5 sec. intervals. S right hand rested on a board with four Morse keys, numbered 1–4 and arranged i an arc.

There were thirty S's, taken from the first- and second-year psychology classes, an divided into three groups of ten as follows.

(1) *The immediate–same* (*IS*) *group* first underwent a habituation phase with stimuli one colour—red for five S's (the 'red subgroup') and white for the remaining five S's he 'white subgroup'). A random sequence of sixty stimulus patterns was presented. f these, the first and second thirty consisted of twelve single stimuli (three in each of le four positions) and eighteen stimulus pairs (three in each of the six possible pairs of ositions). When a single stimulus appeared, S was to press its corresponding key. When vo stimuli appeared, he was to respond to either but not both, in fact to the one he oticed first.

Immediately after the habituation phase followed the experimental phase, in which oth red and white stimuli were used. Eighty stimulus patterns were presented, of which ch successive twenty was made up of eight single stimuli (four of each colour distributed ually among the four positions) and twelve pairs consisting of a red and a white stimulus ach of the twelve possible combinations of positions was used once). The task was as fore.

(2) *The immediate–different* (*ID*) *group* had the same procedure, except that a different sk was imposed for the habituation phase. S was told that either one or two lights ould appear at any one time, and he was instructed to voice a guess during each interval ith respect to the number that would appear next. In the experimental phase, which llowed immediately after, the key-pressing task was used.

(3) *The 24 hour–same* (*24S*) *group* had exactly the same treatment as the IS group, xcept that approximately 24 hr. elapsed between the two phases.

RESULTS

the IS group's experimental phase, the mean number of choices in favour of red imuli was 20·4 for the 'red subgroup' and 26·4 for the 'white subgroup', as compared ith 24 to be expected by chance. According to a one-tailed Wilcoxon test, $T = 16$ and $< 0{\cdot}01$. The colour used in the habituation phase seems, therefore, to affect the choices ade in the experimental phase. A tendency to respond to novel rather than to familiar imuli is thus confirmed in the conditions of the present experiment and may consequently attributed to factors occurring after the onset of the stimuli.

The mean number of choices (out of forty-eight) in favour of novel stimuli made by e IS group (27·0) is significantly greater than the mean numbers made by the ID group 1·0) and the 24S group (22·9): $t = 2{\cdot}10$ and $2{\cdot}24$ respectively, d.f. $= 18$, and $p < 0{\cdot}05$ both cases. The hypotheses that a 24 hr. interval or a change of response between ases will not diminish the attention-to-change effect can thus be rejected, and ex- anations in terms of ${}_{S}I_{R}$ or stimulus satiation *alone* are not borne out.

The failure to find support for the stimulus-satiation theory is interesting in view of veral experiments on maze alternation in the rat (Walker, Dember, Earl, Fliege & aroly, 1955; Kivy, Earl & Walker, 1956; Dember, 1956) which cast doubt on its nerality. The rat appears to turn away from stimuli which it has recently encountered *the same context.*

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A NOTE ON THE FALLACY OF THE 'PHENOMENOLOGICAL FALLACY'

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n a paper in this *Journal* entitled 'Is consciousness a brain process?' Place (1956) claims hat physiologists such as Sherrington, who have recognized the existence of mental or psychical) events *and* physical events, have committed 'a fairly simple logical error' vhich Place calls 'the phenomenological fallacy'. This mistake consists in supposing hat 'when the subject describes his experience, when he describes how things look, ound, smell, taste or feel to him, he is describing the literal properties of objects and vents on a peculiar sort of internal cinema or television screen, usually referred to n the modern psychological literature as the "phenomenal field"'. The fallacy oncerned is 'the mistaken assumption that because our ability to describe things in our nvironment depends on our consciousness of them, our description of things are primarily escriptions of our conscious experience and only secondarily, indirectly, and inferentially escriptions of the objects and events in our environments'. Place says (1) that we do ot describe things by first describing their phenomenal properties and then inferring heir real properties from their phenomenal properties, and (2) that we learn to recognize he real properties of objects before we can describe our conscious experience of them. Ve describe our conscious experience not in terms of phenomenal properties but, 'by eference to the actual physical properties of concrete physical objects, events and rocesses which normally, though not perhaps in the present instance, give rise to the ort of conscious experience which we are trying to describe. In other words when we escribe the after-image as green, we are not saying that there is something, the after-nage, which is green, we are saying that we are having the sort of experience which we ormally have when, and which we have learned to describe as, looking at a green patch f light' (p. 49).

Now there are a number of entangled confusions and mistakes in this argument which shall attempt to unravel. Everyone would agree that it is false to say that when a man s describing some physical object it is really his own experience, or his own sensations, hat he is describing. But we cannot conclude from this that a man can never describe is own sensations, or that when he is describing his own sensations he is really describing hysical objects. For it is true to say that those professionally concerned with the analysis f sensation can take up what Quinton (1955) has called the phenomenological frame of ind and can use words to denote their sensations and can thus describe their sensations vith no necessary reference whatever to physical objects. Furthermore, even when a an is describing a physical object he is certainly also having sensations, which he could ave described instead of the physical object, if he had adopted the special phenomeno-ogical frame of mind and used the 'sensible object' language (or the 'sense-datum' anguage) instead of the physical object language. Now it is perfectly true that the hysical object and the sensible object languages contain many words in common e.g. 'red', 'round', 'ticklish', etc.), but these words are used in each language in perfectly

specifiable ways. For instance, 'this sense-datum is red' describes a fact of direct sensory experience; 'this material thing is red' may be used in a situation where a certain physical object evokes red sense data in the visual field of people under certain specified physical conditions (e.g. the light must be white or red and not blue or green, the person must not have taken santonin, etc.). It is also true that these words have been learned in the context proper to the use of the physical object language and not the sensible object language, for we usually teach children the use of words by showing them physical objects. But this fact has no logical significance, for it is only contingent as the following examples show.

(*a*) A mother could always teach her children the names of colours by reference to after-images and not objects. She could ask her children to look at coloured lights and could say of each after-image as it appeared 'that's *blue*', 'that's *green*', etc. The child could then use these words to refer to the colours of physical objects. Furthermore, a child could learn the proper use of 'green', for example, without ever having seen any green light or green physical object. All we would have to do would be to bring the child up in a red light, and then at a suitable age ask the child to look at a bright red light and say of the resultant after-images in the complementary colour 'that's *green*'. It seems plausible to suggest that after this instruction the child should be able to name correctly any green physical object shown to it in a white light. In which case Place would have to say that whenever the child subsequently looks at a green object and describes it as green, he is *only* having the sort of experience which he had learned to describe as green when looking at a green after-image. So his argument against the thinghood of after-images cannot be upheld on these grounds.

(*b*) Secondly, can we doubt that had hallucinations been very much commoner than they are, and many of them collective (as some are even now), that in this case the use of many words would have been obtained in their sensible object context and not their physical object context?

Now it is clear that the question as to whether an after-image is a thing (or an entity) or not depends on what we mean by 'thing' (or an entity), and we can construct our definition of 'thing' so as to include or exclude after-images as we wish. However, it is important to ask if a scientific explanation of perception should contain 'after-image' as a basic term. In other words do we *observe* after-images? *Do* after-images have the properties they appear to have, i.e. spatial extension, causal relations and colour? Why if an after-image is not a thing, is it so confoundedly thing-like? And if the answer to the first two of these questions is 'No', what is going on when a person claims to be observing an after-image? A behaviourist account would run as follows: 'When a man says he is observing an after-image, all that is happening is that a certain excitation is reverberating in his retinal and visual cortex leading to correlated excitation of the motor speech centres via the computing and communicating mechanism connecting the two. Thus certain noises are made that may be depicted by the symbols "I am seeing an after-image". On the other hand, no such utterance may be made at the time, but a memory trace of the reverberating activity may be laid down so that a subsequent statement "Last week I had an after-image" may be made.'

The behaviourist would claim that the after-image does not have the properties it appears to have, for all we are dealing with are the verbal responses of organisms which use a misleading 'object' language when their outputs are activated by certain interna

vents in their mechanism. The organism should not issue the symbols 'I am observing n after-image', or even (vide Place) 'I am now having the sort of experience that I ould have had had I looked at such and such a light', but only 'I am now making the rt of statements that I would have made had I been observing such-and-such a light'. he alternative account states that images are a part of the fundamental furniture of ie mind and do have the properties that they appear to have.

Now it is not necessary to discuss here which of these two accounts is *correct*. I only ish to point out that neither makes any logical mistake. They are equally valid ntologico-empirical statements. One says there are only physical objects. The other ys there are physical objects *and* images. The only way in which we could possibly ecide between them would be by experiment. I have suggested elsewhere (1956) how is problem could be approached.

Now Place says that an after-image is not merely what a red light of a certain kind ould have looked like had we seen it, but that in the case of the after-image we are aving an experience similar to that which we would have had if we had looked at a rtain light. To this we can say that it is difficult to see to what this term 'similar xperience' refers in such a usage. 'Experience' usually refers to *events* experienced. or example, in the common-sense usage, 'I had a similar experience ten years ago' eans that ten years ago I observed and lived through certain events of similar kind those under consideration; or, in the technical phenomenological usage, it means that rtain phenomenological (experiential) events occurred in my sense fields ten years ago.

But it seems odd to say that we could have an experience of *nothing*. Surely to experience othing is not to have an experience? Surely a sensory experience is nothing more than certain group of events, i.e. the causal processes of perception both outside and inside ie organism in the more extended use of 'experience' ('I have had some interesting xperiences in my time') and certain experiential events in the technical phenomenogical use of 'experience'. It would surely be a mistake to say that we are dealing with ie causal chain *and* the experience, so that we could logically have no events and *yet* ave an experience. For this is clearly the same category mistake that Ryle claims the artesian dualists make over 'mind'. To this Place might reply that, in the case of the fter-image, certain cerebral events were going on to cause the experience. But surely is only clumsy and misleading to speak of cerebral events, or of events in the perceptual echanisms, as causing (or being correlated with) the 'experience of nothing' when the xperience is nothing more than the physiological causal chain. The physiological causal hain cannot cause the experience—it *is* the experience. In which case, we do not have use the language of experience at all but only the language of the radical behaviourist heory.

Thus we can say that it is certainly false to say that, when people are describing hysical things, they are really describing their sensations. But it is also false to say hat, when people are describing their sensations, they *must* really be describing the hysical things causally related to their sensations, or the sort of things that would be ausally related to their sensations, and so on. Furthermore, it is a contingent fact that ie use of words is obtained by reference to physical objects and we can easily imagine least some instances where this would not be so.

So we may conclude that the statement 'there are after-images and they are green ntities' is not derived from any logical fallacy but it merely expresses one particular

ontological theory about images, and the statement 'there are no after-images' merel expresses another ontological theory about images and neither can be proved or disprove on logical grounds alone.

I am most grateful to Prof. O. L. Zangwill, Mr James Thomson and Mr Richard Gregor To them are due whatever backbone or finesse this note may have.

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PUBLICATIONS RECENTLY RECEIVED

Study of Thinking. By J. S. Bruner, J. J. Goodnow and G. A. Austin. New York: Wiley; London: Chapman and Hall. 1956. Pp. xi + 330. 44*s*.

his book must be reckoned one of the most important contributions yet made to the study of concept rmation. It breaks away completely from the problems which dominated investigation into this subject the twenties and thirties (e.g. whether concepts can be formed unconsciously, what sort of images company the formation of a concept, etc.); such problems have revealed their sterility, and have been ven a decent burial in the pages of Humphrey's book on 'Thinking'. The present authors are of course t the only workers in the field who are employing a new approach to the study of concept formation; e book is very much the product of the *zeitgeist*, and draws heavily on concepts developed in games eory and information theory.

The question to which this book attempts an answer is 'How do we form concepts?' The authors have ne some way towards giving a new answer to this problem by taking two important steps. First, they use e rudiments of games theory to analyse what methods we might expect to be used in forming concepts d how these methods might be expected to vary with variations in the conditions under which concepts ight be formed. The methods used to arrive at concepts are termed 'strategies'. Secondly, in the periments reported, the authors have developed greatly simplified experimental techniques which able them to analyse the strategies with which their subjects are operating.

The easiest way of illustrating these two points is to outline part of the experimental technique. n array of sixty-four figures was set out before the subjects; it was explained to the subjects that each ure exemplified one of two possible values of six attributes, and subjects were shown what these tributes and their values were. Subjects were then told that they had to form a concept; figures which re an instance of that concept could be defined by certain common attribute values. All subjects were en shown a positive instance of the concept, and told to arrive at the concept by choosing further ures to see whether they were or were not instances of the concept. It should be noted that the great vantage of the experimental technique used is that, because there are only a finite number of possible ncepts and all these are made clear to the subject before the start of the experiment, it is possible to rform the following types of analysis on the data: one can say how much information a subject can quire by any given choice and compare this with the amount of information he makes effective in his esses; knowing the choices a subject has made, it is possible to say what further choices would extract e maximum amount of information from untested figures and to see how far a subject's actual further oices satisfy the condition of obtaining the maximum amount of new information; it is also possible to ate how a subject's choices depart from random choices.

The authors give a subtle analysis of factors likely to influence the pattern of successive choices; thus, addition to regulating the risk of failure to attain the correct solution in a given number of choices, they int out that certain strategies can facilitate the task of assimilating and storing information (e.g. nsidering only one hypothesis at a time and relating information received only to that hypothesis), d they are able to show that a strategy which makes the assimilation of information easy is proportion- ely more successful as the experimental conditions render it more difficult to assimilate and retain formation (e.g. they compare conditions where subjects can see the results of all past choices with nditions where they can no longer see the results of past choices but must store the information in their ad if they wish to use it).

As well as investigating how subjects select figures to attain a conjunctive concept, the authors have vestigated the strategies used when subjects are given no choice of the order in which positive or gative examples of the concept are presented to them; strategies are revealed by forcing the subjects write down what they think the concept is after each example has been presented. They have also vestigated the attainment of disjunctive concepts, i. e. the formation of a concept consisting of a dis- nctive combination of attribute values. A further major novelty in their approach is that they attempt investigate categorizing where the cues to be used in the categorizing are only probabilistic. This, of urse, has been done before, but the authors try to give a rationale for matching event probability in quency of guessing the event, and to show under what conditions such matching takes place.

A large number of variables affecting the kind of strategy which will be adopted, and the difficulty of rrect concept formation are subjected to theoretical analysis in terms of the concepts of games theory,

and many ingenious experiments are reported to test predictions arrived at in this way. It is clear th the authors and other recent workers in this field have opened up an entirely new range of problems, ar a very promising field for further research. The authors make it clear throughout that the type of the retical analysis they are employing opens up far more problems than they have attempted to solve; t important point is, that by developing a new way of analysing the problem of concept formation and ne experimental techniques, they at the same time raise new problems and provide a method for solvi them. In a way, the sort of explanation given throughout the book is like the explanations of behavio given in everyday life—the main explanatory concepts employed are aims and expected outcomes. T virtue of games theory is that it has provided a technique for the quantification of such concepts, ar offers the psychologist some hope of being able to give this type of explanation and at the same time ma more or less rigorous predictions. This book is the first large-scale attempt to apply this type of analysis individual human behaviour, and to test the extent to which the facts of human behaviour can be fitt into the new conceptual framework; as such it is a portent of things to come. It suggests a way of deali with those areas of behaviour which are too complex or about which too little is known to make it wor while casting hypotheses about them in a mechanistic or physiological form.

The book has only one serious fault: the exposition is not always as clear as it might be, partly becau the writing is often clumsy and partly because attention is distracted away from the main argument b a series of comparisons with concept formation in everyday life which are often more misleading tha helpful. For example, the difference between those who believe in localization of function in the bra and those who do not is far too complex for a comparison between the thought process of the two schoo to form a good and helpful illustration of what the authors mean by wholist and part strategies.

There is a 65-page appendix to the book written by R. W. Brown in which he attempts to show that t way of looking at concept formation developed in the body of the book can be applied to the formation linguistic concepts and to the formation of concepts in the world to which words apply. Though he giv an admirably clear account of some of the psychological problems to do with language, the connexi between his account and the main body of the book remains tenuous, and little effort is made to sho what light is thrown on the problems of language by using the theoretical analysis and experiment techniques developed in the remainder of the book.

N. S. SUTHERLA

Perception and the Representative Design of Psychological Experiments. By Egon Brun wik. Pp. xii + 154. University of California Press; London: Cambridge Universit Press. 1956. 37*s.* 6*d.*

It is some slight consolation for the loss of Egon Brunswik that he should have left behind this compr hensive statement of his views. No one interested in psychological theory can afford to ignore it, ev though at times it may make unusually heavy demands upon his penetration and patience. For Brun wik could only express his ideas in the abstract idiom of a German philosopher, and the ideas themselv are not easy. His last sentence may serve to illustrate his style: 'Preception, then, emerges as th relatively primitive, partly autonomous, institutionalized, ratiomorphic subsystem of cognition whi achieves prompt and richly detailed orientation habitually concerning the vitally relevant, most dist aspects of the environment on the basis of mutually vicarious, relatively restricted and stereotype insufficient evidence in uncertainty-geared interaction and compromise, seemingly following the highe probability for smallness of error at the expense of the highest frequency of precision.' But abstract a clumsy though this sentence may seem, it is not the work of a man whose primary concern is to expou a theory. Rather it is an effort to present the many-sided nature of the perceptual process as it reve itself to a genuinely empirical study. It is we who become theoretical when we simplify our data Lockeian or Gestalt terms.

The book is divided into two parts, of which the first was printed in the *Berkeley Symposium Mathematical Statistics and Probability* in 1949. The second part, entitled *Perception, the Ecologi Generality of its Distal Aim*, does little more than develop and illustrate some of the earlier ideas.

The argument of the whole book is based upon the account of perception which has been quoted abo If the sentence is studied adjective by adjective, it will be seen that a very wide range of experimen material is being taken into account. Ames, Bruner, Erismann, Gibson, Helson, Lashley, Tolman a Werner are among those whose work is relevant.

Now, if perception is the sort of thing it seems to be, then how can we study it? Brunswik provide new analysis of experimental method in psychology, and shows the defects of, on the one hand, t traditional experiment with its manipulation of a single variable, and, on the other, the multifactor

pproach, using analysis of variance in situations which have been artificially randomized. If perception ere a purely causal affair (if, that is to say, stimuli *made* us perceive instead of *enabling* us to perceive), ien the first kind of approach would be appropriate, as, indeed, it still is in sensory physiology. If the iances and events of this world came along as systematically as the plots of an agricultural experi- enter, then we have statistical techniques which we could use. But perception and behaviour, and deed the whole process of living, seems to fall somewhere between these two extremes of rigidity and ndomness, and, on Brunswik's view, we need a third method to handle the sort of data prevalent in ology. In spite of some ingenious approximations, Brunswik admits that, although we can point to the rt of method we need, we do not yet have the mathematical tools which would form an essential part of it.

The further stages of the argument may seem open to criticism. If, as would seem to be the case, our rceptual world is an achievement made possible by cues which can deputise for one another and which ay be differently weighted in every concrete preceptual situation, then the task of designing a 'repre- ntative' experiment appears to be impossible. Indeed, the whole distinction between an experiment in hich the variables are known and controlled, and a real-life situation, tends to become blurred. Even lowing that we must sample a far wider range of possibilities than those normally present in a psy- ological laboratory, the decision as to whether a particular experiment can be regarded as 'representa- ve' seems to remain, to some extent, arbitrary. What we have instead is an observational approach, ing measurement where possible, but producing in the end a functional and descriptive account of rception rather than a causal analysis. It may be that this is what we should aim at, and it is certainly mething worth doing from the point of view of biological science as a whole. But one is reluctant to cept the claim that this is the only way in which perception can be studied.

One may gain much from this book without accepting its general thesis. The acuteness of Brunswik's alysis and the obstinacy of his struggle with fundamental issues arouses admiration. If the text may metimes seem perversely complicated, one is never quite sure that it is the writer who is to blame rather an his data. It is difficult to see how any future psychology of perception can avoid dealing somehow th the issues which Brunswik has raised.

JAMES DREVER

ocial Structure and Personality in a City. By O. A. Oeser and S. B. Hammond (Eds.). London: Routledge. 1954. Pp. xxii + 344. 30*s*.

his is the first of two volumes edited by Oeser and Hammond. The second volume, to be reviewed later, escribes social structure and personality in a rural community. The studies were undertaken by the epartment of Psychology at Melbourne University from 1947 to 1950 as part of an international study communities and social tensions commissioned by UNESCO.

In the preface of this volume, Prof. Oeser explains how successive class projects carried out by the llective Behaviour course were used as a basis for much of the material and gave leads for conceptualiza- on. These studies were considerably enlarged and systematized when the UNESCO grant became ailable. In the first chapter, the authors explain the orientation of the book: '...we have chosen to al with topics concerned with the behaviour and outlook of individuals in a life context'. Historical, onomic, ecological or political facts have been mentioned only in so far as they affect the individual. imarily 'the inner, partly autonomous, psychological structure and dynamics of individuals' has been udied, 'only in so far as it affects their common every day living and beliefs. We are concerned with the terminants of social behaviour rather than with its remote or intrapersonal conditions.' The problem th which the writers deal has 'three aspects', i. e., 'the enunciation of sociological facts, the enunciation psychological facts, and the investigation of the way in which the psychological facts rest upon the ciological'.

The book is divided into four sections. The first describes the Australian's attitude to his own and to her countries, his attitude to immigration and, in particular, his attitude to Jews. The description is sed upon data obtained from Gallup polls and from studies carried out by the department. One of the udies deserves special mention, namely, that of relating the perception by Jews of the existing degree anti-Semitic feeling to that degree of feeling actually elicited from non-Jewish respondents. This ction is carefully planned and the discussion based on a wide range of data. It suggests useful leads r future inquiry.

The second section deals with the interaction pattern of the members of the family. It is based on a ghly elaborate conceptual framework, confusing to the reviewer at least, and rendered more so by the pological models used. To test this conceptual framework, 128 11–12-year-old school children are ked in a group test to note which member of the family usually does each of thirty-three activities

listed: 'Who usually does each thing, who helps, and who would never do it.' They then go over the activities again and are asked the following: 'It is quite likely that your parents do not always agr about all these things, so we want you to tell us how often they disagree.' The child is given a choice categories—Often, Sometimes, Never—which he is asked to tick. The items range from 'Who decid when broken things should be repaired' and 'Which of your parents decides when to visit your mothe relatives' to 'Who decides how much money should be spent during the holidays'? No explanation given for the selection of these activities, no doubts raised as to whether, for example, the child is real in a position to know who decides how much money is to be spent during the holidays.

On the basis of the answers to this group test and with no further check by relating these answers those obtained from parents concerning these activities, the families are divided into four group husband-dominant, wife-dominant, autonomic (where each marital partner has autonomous areas control) and finally the syncratic co-operative household in which husband and wife act and deci together. Further, each family is given a tension index based on the number of times the child has tick that his parents sometimes or often disagree. These data in turn are related to age at marriage, occupation status, as well as signs of early maladjustment in the child (obtained from interviews with the mothe and to present signs of maladjustment in the child. A tetrachoric correlation of 0·078 between ma adjustment and type of family structure is reported. No doubts are expressed concerning the validity this unusually high positive relationship.

Some of the findings are both interesting and important, but to assess their value, further resear would have to be done with techniques which depend less upon the child's perceptions of the situatio It is a pity that no mention was made in this work to the relevant studies carried out by Fels or Symond

The third section deals with the child and is based on tests given to the 128 children and on intervie with the parents of these children as well as on information collected from Kindergarten teachers on ea training. These studies examine the behaviour expected of the child, and the techniques used in trainin as well as the different roles which boys and girls are called upon to play.

The school children are questioned on the same areas of behaviour, using Bavelas Home Ideolo questionnaire. Very interesting findings emerge, such as the relative degree of emphasis on conformi expected from boy and girl, the space of free movement allowed and the types of controls exerted. would have been interesting, had time allowed, to relate the information obtained from parents to th obtained from the child. On the basis of TAT stories, the children were rated in terms of their adjustme to the family, a score which was then correlated to peer adjustment, to family tension and to fam structure.

The fourth section deals with the place of work in social life, with class and family, with the relation class membership to child training and with the effect of the individual's perception of his class memb ship on his behaviour and on his views on a variety of issues. This section is by far the best in the boo It shows the value of the multidimensional approach that has been adopted, namely, to collect a larg amount of data about the same families which makes it possible to determine the interdependence attitude and behaviour. References to such obviously relevant studies as those of Havighurst and Da are lacking.

The book is undoubtedly valuable. It discusses novel techniques of inquiry and attempts conceptuali tion. It is also perhaps rash and somewhat uncritical of the instruments used. Nevertheless, desp these deficiencies, perhaps inevitable in a pioneer work, it will occupy a significant place among t studies of child training and family life.

HILDE HIMMELWI

The Social Problem of Mental Deficiency. By N. O'Connor and J. Tizard. Londo Pergamon Press. 1956. Pp. viii + 182. 30*s*.

The main purpose of this book is to give a summary of the research carried out over the last eight years members of the Medical Research Council's Social Psychiatry Research Unit. Drs O'Connor and Tiz have, however, done much more than this in presenting the results of surveys and investigations wit a framework of carefully collated statistical material culled from public and departmental reports. T outcome is a very clear and well-written account of the present status of certified mental defective England, and there is no other book where this factual material is so readily available or so cle summarized. The title, however, is not an entirely happy choice, for, as the authors admit, their stu have been concerned more with occupational than social problems, and further information about position of the mentally deficient person in the community is greatly needed.

The researches have usually involved two main stages: first, a broad survey of the particular field,

condly, detailed experimentation upon a salient aspect of it. This has proved most fruitful. Unlike the edical investigations of the pathological aspects of mental deficiency, social surveys have, with the ception of the classical pre-war work of Burt, Lewis and Penrose, hardly been touched upon. In view the radical changes in social and economic conditions following the war, a reassessment of this field was eatly needed. The book gives an account of the prevalence of mental deficiency and the services ailable to deal with the problem. Psychometric surveys yielded the surprising information that about lf the largest clinical group, the feeble-minded, had I.Q.'s over 70 points on standard intelligence tests; is indicates that the legal concept of mental deficiency has widened since the time of the Wood Report. mewhat unexpected results also emerged from experiments on learning in imbeciles, and these have portant implications for occupation and training centres.

An account is given of various psychometric theories of stability and neurotic tendency among defec- es, together with investigations into the prediction of occupational success; the chief finding was that sts alone are not valid predictors and that economic and social factors play an important part in ccess or failure, as would be expected. The section on occupational adaption in the community represents beginning in a relatively unexplored field, but the findings must be regarded as tentative since the llow-up period was short. Certainly future work will have to concentrate more on this since most high- ade defectives are to-day dealt with not primarily because of occupational problems but on account of ner social difficulties. The status of the imbecile in the community also requires further elucidation.

Part of the book deals with experimental studies of occupational training, education and psychotherapy th unstable patients, all of which yielded hopeful results. The authors conclude with a chapter on ministrative and legal implications and offer some stimulating suggestions for changes in our methods dealing with the mentally deficient. This section is somewhat controversial, particularly the argument at a special Mental Deficiency Act is no longer needed and that those at present certified could be dealt th by other existing services.

Partly because of the change in national socio-economic background, and partly stimulated by the dings of the M.R.C. Unit and other research workers, the approach to the problem of the mentally ficient is changing, and the custodial attitude is gradually being replaced by a more remedial one, or at st one which concentrates its efforts on capitalizing on the assets of the subnormal and making the best e of limited potentialities. Drs O'Connor and Tizard have demonstrated that this is a field in which cial scientists can play an important and rewarding role. In a world where psychologists, psycho- erapists, physicians and educationists have been and are only too prone to write-off the subnormal as interesting diagnostically and unrewarding prognostically, this book provides a refreshingly novel proach to the problem, and also a warning to those who are content to generalize on the basis of stereo- pes which arose without adequate experimental observation in different socio-economic circumstances m those pertaining today.

A. D. B. CLARKE

redgold's Textbook of Mental Deficiency. By R. F. Tredgold and K. Soddy. London: Baillière, Tindall and Cox. Pp. xvi+480. 40*s*.

this ninth edition of his father's original text, Dr R. F. Tredgold has succeeded in carrying on a notable dition. With Dr Kenneth Soddy's help he has presented the point of view so well known since the first tion, and found also in the late Dr A. F. Tredgold's other writings. In certain ways, therefore, apart m modernizing detail (e.g. in the chapters on Pathology), the authors have not attempted to reorganize e available knowledge as an entirely new work.

The character and presentation of material differs from the previous (1952) edition in its treatment of chology, and the social and administrative problem of mental deficiency. Some chapters, such as that the Normal Mind, do not present anything surprisingly novel as compared with previous versions. scussions of Conation, Affect and Cognition follow previous trends relying on Spearman and McDougall. wever, in the next chapter, 'The Defective Mind', an entirely new approach to the problem of instability defectives is suggested by one short section entitled 'Damaged' Children. This section reports some of e early work of Spitz and Bowlby and the work of Roudinesco from Dr Soddy's recent book on Mental alth and Infant Development. In a broad and general way, an explanation of 'moral deficiency' as ined in the Mental Deficiency Acts is offered in these terms, but the more recent work of Bowlby, and urne's *Lancet* (1955) article are not mentioned. No criticisms of the hypothesis of emotional depriva- n are discussed.

Dr Tredgold and Dr Soddy examine the Sociology of Mental Defect in a chapter the latter part of which been revised. The revision does not mean a change of approach, and the stress is still on the problem

of the presumed increased incidence of mental defectives. The opinions expressed by Dr A. F. Tredgol in the columns of the *Eugenics Review* for 1908 are found again in this chapter, namely, fear of the increa of mental deficiency and fear of resulting national degeneracy. The authors do not expect that euthanasi sterilization or segregation will help to solve the problem, and they devote their last chapter to a limite survey of methods of treatment which may give hope for the alleviation of the symptoms of mental defec

This text needs to be assessed from the medical as well as the psychological point of view to do justice. The authors disclaim any intention of offering an adequate description of the psychology mental deficiency, and do not present any results of current psychological research. Its use to psychologis will therefore continue to be as a compendium of clinical medicine. In reading it, they must be caref to distinguish clinical impression from experimentally tested knowledge. In the field of mental deficienc because of the lack of research, the former is often presented as the latter.

N. O'CONN

Studies in Feeling and Desire. By J. C. Flugel. London: Duckworth. 1955. Pp. xv 200. 16*s*.

In its 'collected papers' quality, this book resembles *Men and their Motives* (1934) rather than *Ma Morals and Society* (1945). Yet, like all the late Prof. J. C. Flugel's major works, there is a central unit in this instance it is one which will be familiar to those of us who had the good fortune to attend his cours on orectic psychology. It is 'feeling' (in the sense of pleasure—unpleasure) rather than 'emotion' whi forms the central theme of these essays. The author is never happier than when dealing with the maj complex and subtle problems in this area; he examines their psychology with a surer touch than any oth authority with whom the reviewer is acquainted.

The five chapters comprise: I, Sublimitation, its Nature and Conditions; II, Feeling and the Horn Theory; III, *L'Appétit vient en Mangeant*; IV, The Death Instinct, Homeostasis and Allied Concep V, A Quantitative Study of Feeling and Emotion in Everyday Life. The first four of these papers a mainly theoretical and Chapter IV in particular has been considerably rewritten since its original publi tion. In Chapter I the author deals with sublimation in relation to 'more familiar' concepts like associati learning and conditioning. The succeeding two chapters, replete with stimulating thought and insigh lead up to the treatment of the death instinct; this is considered in terms of the idea of such vari authorities as Fechner, Claparède, Zeigarnik, Cannon (homeostasis), Wiener (Cybernetics) and Z (principle of Least Effort). In this fourth chapter and in Chapter III—the two which the reviewer found t most fascinating in a stimulating book—Flugel presents his own development of ideas deriving fr James Drever (Sen.) concerning tension-reduction in its relation to the self-sustaining tendencies motivation.

In this book we meet some of Flugel's most characteristic ideas and insights: his concern with formu tion of laws of feeling; the principle of increase of satisfaction through inhibition (prominent in *T Psychology of Clothes* (1930) and *Men and their Motives*); his critical approach to Freud's concept of t death instinct, reformulated in terms of Cannon's homeostasis; and his concern to integrate the researc of Wolgemuth and numerous other investigators of emotion and feeling, who worked under his directi Characteristic of this book is its concern with definition and greater methodological rigour of the ki which Flugel, like Ernst Kris and a few others, have sought to bring to psychoanalytic thinking. Abil to speak the languages of both what Cronbach (*Psychometrika*, 1954) has recently called 'Clinicia' and what he has called 'Psychometrica' contributes to the strength of this book; its author does more th merely speak both languages fluently; he writes with the authority of being himself an experimental of stature together with the scholarship of an historian of psychology.

The originality, breadth and depth of Flugel's thought recommends this book; because of the richness its insights and the reflection of its author's humanity, scholarship and wisdom, few psychologists c afford *not* to read it. The appearance of this book marks the passing of a major psychologist, and remi us that its author, John Carl Flugel, has for many reasons, a secure place in the history of the scien

P. MCKELL

Developments in the Rorschach Technique. Vol. II. *Fields of Application.* By Bru Klopfer and sixteen contributing authors. London: Harrap. 1956. Pp. xx + 828. 5

Because of the high level of one's expectation, it was perhaps almost inevitable that vol. II of 'the n Klopfer' should prove something of a disappointment. Unlike its predecessor (reviewed in this *Journ* 1955, **46**, 72–3) the present volume follows very closely the current fashion for publication in symposi form. Consequently, while there is a quite remarkable unity of theoretical orientation, the diversity

reatment is at times disconcerting, particularly in the case of contributions which bear signs, as some o, of having been prepared for use in some other context. Further, not all the contributing authors rite equally well, and the reader who is sensitive to language may find himself wincing from time to time. In his Foreword, Bruno Klopfer concedes the point regarding diversity; he also admits that he and his o-authors are 'just beginning to pile up convincing evidence for the statements made'. Edwin Shneidan, again, in his chapter 'Some relationships between the Rorschach technique and other psychodiagostic tests', opens with the remark that 'the exact relationships among psychological tests are almost ntirely a matter for future research'. The cynic will no doubt say that the truth of both statements is all oo evident, and suggest that the present publication might well have been postponed. If one looks to ne book for 'all the answers' one may be tempted to agree with our hypothetical critic. Nevertheless, s four parts, each devoted to a broadly defined 'field of application' (Genetic Psychology; Medical sychology; Social Psychology, Anthropology, and Industrial Psychology; Diagnostic Practice and rojective Theory) contain much that is of value, not only in terms of Rorschach practice, but also in elation to the general study of personality in its varying aspects. Particularly interesting, from both oints of view, are Gertrude Baker's chapters on 'Diagnosis of organic brain damage in the adult', and ertrude Mieli-Dworetzki's on 'The development of perception in the Rorschach'. This latter is essentially research report with normative data, which, while simply presented and conservatively interpreted, re adequate and convincing.

A third disclaimer, amounting almost to a main theme of the book, is summarized (by Hedda Bolgar) the concluding sentence of the last chapter: '...we do not know too clearly what the boundaries re between the stimulus effect of the test material and the stimulus effect of the examiner....' This cognition of the test situation as a social or even therapeutic relationship represents a considerable dvance in projective practice, and forestalls a good deal of criticism impugning dubious reliability and alidity. In this connexion the reader is referred to Shneidman's chapter, already mentioned. Contrary beliefs entertained in some quarters, Rorschach practice does seem to be moving in the direction of ehavioural analysis and away from arbitary systems of interpretation from 'signs'.

The book ends with an elaborately classified Bibliography extending to over 100 pages, together with Cumulative Indexes' covering both volumes.

B. SEMEONOFF

earing Defects in School Children Scottish Council for Research in Education. 1956. Pp. x+112. 10*s*. 6*d*.

nvironment and the Deaf Child. By Steven Getz. Illinois: Thomas; Oxford: Blackwell. 1953. Pp. xvi+173. 27*s*. 6*d*.

its latest publication, the Scottish Council is concerned with two problems: (1) the incidence of hearing efects among school children, together with their distribution and classification; and (2) the educational nplications of these hearing defects. A complete year group (12-year-olds), in all 4170 children, were amined. The defects of hearing were ascertained by means of a pure-tone audiometer test carried out in o stages. Every child in the sample took part in a sweep test. Each child was tested at the 10 db. tensity level on the four frequencies of 250, 500, 1000, 2000 c.p.s. All those who failed at the 10 db. level any one of these frequencies were given the full pure-tone threshold test of eight frequencies through 5 to 8000 c.p.s. In determining the relation between hearing defects and educational attainment, the sults of the transfer examination test were used (Intelligence, English and Arithmetic), since results in ese tests existed for nearly every child in the group. The investigation revealed many points of interest, d these are all clearly laid out in this book. Quite apart from its main aims and findings, this book is of ecial value for the many incidental but important points which are discussed; for example, the problem differences in calibration among pure-tone audiometers, the question of unreliability among testers even th such an objective instrument as an audiometer, differences revealed in hearing loss on a re-test, after veral weeks, the factor of noise in the examination of children in school, and the awareness or otherwise teachers of hearing defects among their pupils. The report concludes with twelve recommendations, for ch of which the investigation itself provides positive evidence. This book can be strongly recommended educational and medical authorities who are planning pieces of work in their own areas.

The author of the second book is a psychologist in a school for the deaf in California. Getz took a group 'relatively well-adjusted students and then a group of relatively maladjusted'*. These two groups

* Assessing adjustment by the Haggerty–Olson–Wickman Behaviour Rating Schedule A, and the Brunschwig rsonal Adjustment Inventory for Deaf Children.

were compared for each of the variables he proposed to study, his aim being to note any difference between the two groups on each of these variables. The variables were age at entering school, ability t communicate with the hearing and with other deaf pupils, educational achievement, social maturity reading achievement, visual functioning and intellectual functioning. His conclusions for each item ar briefly noted and discussed, and, in perhaps the most valuable part of the book, he discusses the argu ments for and against the more controversial points held by other workers in the deaf field. The book is careful but rather condensed study of the subject, and is suggestive rather than conclusive. Its 30 page of bibliography will be of value to students in this field, and the book can be recommended as a usefu synopsis of certain current problems in the education and upbringing of deaf children.

The Scottish Council's book scores by taking a relatively limited problem and treating it thoroughly Getz, on the other hand, seems to be less convincing, perhaps because of the extent of ground he covers but his book is full of ideas, and it certainly reveals the liveliness and sharpness of the controversial issue which circulate in the world of deaf education today.

H. H. HOO

Non-parametric Statistics for the Behavioural Sciences. By Sidney Siegel. London an New York: McGraw-Hill. 1956. Pp. xvii+312. 49*s*.

The publication of this, the first comprehensive text dealing with non-parametric statistical method comes most opportunely at a time when the forthcomong Fifteenth International Congress of Psycholog announces 'Scaling methods and non-parametric statistics' as one of its symposium themes. Dr Siegel book, however, is more than a compendium of statistical techniques; it provides an exceptionally cle and stimulating account of the aims and methods common to all statistical treatment of psychological data

In his brief discussion of the theory of measurement the author distinguishes between four 'level of measurement, to the two more primitive of which the term is not, perhaps, usually applied. Measur ment on the nominal scale would more commonly be described as classification, and that on the ordin scale as ranking. It is with these types of measurement that non-parametric statistics is almost entire concerned. Reference to the useful summary table of available statistical tests, which is convenient printed as end-papers, shows that when one reaches the level of the interval scale (i.e. measurement as is usually understood, though not necessarily with an absolute zero) available non-parametric metho cease, for most purposes, to be specially appropriate, in so far as they fail to make use of all the informa tion contained in the data. Thus, there is no non-parametric measure of correlation which will hand data provided at the interval level; consequently, if the nature of one's population distribution does n conform to the requirement of normality assumed in the use of product-moment correlation, one has choose between forcing the data into form by adjusting one's scale interval or losing information (a 'power') by falling back on a non-parametric test based on ordinal measurements—alternatives neith of which will appeal to many psychologists. Since the orientation of the present work is almost entire towards hypothesis testing as the primary function of statistical investigation, the drawback is not serious after all, at any rate in this context. However, the researcher who has to handle interval-sca measurements which conform to unusual distributions (e. g. J-shaped) will be disappointed—having bee misled, perhaps, by the term 'distribution-free', which Dr Siegel wisely avoids—to find that the range problems for which solutions are offered is very limited. Furthermore, one's eye having been caught b the recurrent heading 'Example for large samples', one learns with dismay that to the non-parametr statistician a 'large' sample is one in the neighbourhood of 20.

All this is intended not to imply criticism of Dr Siegel's excellent book, but to call attention to the yet limited scope of non-parametric methods. It might indeed be preferable to make freer use of th term 'ranking' methods, as Kendall does—although this would not cover such techniques as the Wal test for equivalence of related samples.

The many tables necessary for the application of the methods discussed are all contained in th Appendices, which extend to more than 50 pages. To have collected these, many of which have hither been relatively inaccessible, is in itself a service of inestimable value.

B. SEMEONO

Mr Lyward's Answer. By Michael Burn. London: Hamish Hamilton. 1956. Pp. 28 21*s*.

This is an account written by Michael Burn of the work carried out in the residential 'school' of Finchd Manor, Kent, a school for maladjusted adolescents, created by Mr Lyward and which is under his sup vision and care. After fifteen years of teaching experience, the helping of difficult boys became his lifewor

About forty youths, ages 15–20, live together at Finchden at any one time, some sent privately by arents, others paid for by the local authorities. The atmosphere is one of comparative freedom and little straint in which the boys find respite from the various pressures of life, and gradually return to normality. lost of the boys have been under the care of different types of specialists, and seem to be sent to inchden as a last and desperate resort.

It is encouraging to find that, while some failures are reported, the majority of the boys who have assed through Finchden have made good in later life, and, in some cases, have had distinguished careers. he book contains descriptions of the boys and their difficulties, and one senses their struggles, and the terplay of personalities in the incidents and conversations recorded. One also catches glimpses, now and gain, of the seemingly casual treatment given by Mr Lyward, who gains the respect and affection of even ie most hardened of boys. The details of one boy's problems and treatment make instructive reading.

The book is not systematic nor factual, and the first few chapters are disappointing in that they produce somewhat confused picture of Finchden Manor and of its inhabitants. Some of the later chapters, owever, are clearer cut and of greater value to the specialist, for, in these, an attempt is made to sort ut some of the impressions of the writer and to assess the treatment. One cannot but admire the hard ork of Mr Lyward and his staff, which is so carefully, if rather picturesquely, described by Mr Burn. he chapters as they unfold tell the same kind of story that other fairly similar places tell of children and ouths with difficulties which they are unable to overcome by their own efforts. The book, as a whole, is greater interest to the general reader than to the specialist. The latter, however, is happy to know that ich excellent therapeutic work is being undertaken, and is appreciative of the manifold difficulties herent in such a task.

MARY COLLINS

Insettled Children and their Families. By D. H. Stott. London: London University Press. 1956. Pp. 240. 16*s*.

his is a book which should have popular appeal for Probation Officers, Social Welfare Workers and eachers. The type of case-work analysis suggests the delinquent child and his background.

The book is divided into two sections, the first being an expansion and enlargement of two lectures ound which, the author tells us, the book ultimately grew. In this first section an examination is made the effectiveness in practice of the social theory of humanitarianism, followed by criticisms of the esent procedures for dealing with unsettled children. It is, however, questionable whether, in a book signed for a wide lay public, it is professional or suitable to present the author's criticisms of his own ofession and their methods. One feels he is being less than fair to his colleagues. He stresses the need to al with the family as a unit before we can deal with the individual child, and the importance of the study relationships within the family. The science of family case-work is then discussed and an outline is given some of the causes of breakdown in family life, for the guidance of Social Workers and others. He shows nsettled behaviour as a series of typical patterns, each representing stages of maladjustment. This is a seful chapter and contains much commonsense and helpful advice. A further examination is made of lationships within the family group, and a thesis is presented that, basic in all the breakdown patterns, the feeling of the child, that its membership of a family group is in jeopardy. Twelve standard ways e then presented in which a child's security can be threatened. Consideration is then given to the inciples of helping parents to understand why things have gone wrong. In his enthusiasm, however, prove that emotional and environmental factors underlie all motivation and complications of family fficulties Stott denies intelligence a place. This assertion might be dismissed by many psychologists ith long experience of dealing with problem families. A further chapter gives examples of cases with eir methods of diagnosis and treatment.

In Section II of the book, an examination of his standard family patterns is made, and he returns to e conclusion that the solution, in helping these children, lies in understanding that their feelings of privation exist because they have no reliable group to which they can feel themselves permanently tached. The plurality of causes of delinquent behaviour is stressed frequently. The wider social causes of mily breakdown are examined and finally he presents a theory of maladjustment. In this theory, nich is reminiscent of McDougallian Instincts, the key to understanding human behaviour lies in the xecutive reactions' which to a certain extent he equates with instinctive reaction.

Family History Charts are given at the end of the book, and in the Appendix syndromes of parent-ild attitudes which are useful for reference.

This is a book in which the author, although he says little that is new, has made a courageous attempt classify, so far as possible, some of the environmental situations which lead to juvenile breakdown in

behaviour and delinquency. This is a very difficult task, by reason of the width of the field; but if enviro ment is not only exogenous but endogenous where do the claims of heredity come in? There is also danger that the findings of psychology can be considered too simple and too direct. There are mar controversial statements in the book, but it contains much commonsense and presents a realistic approac in trying to solve very complicated problems. J. C. MACLEA

Personnel and Industrial Psychology. Second edition. By Edwin E. Ghiselli an Clarence W. Brown. New York: McGraw-Hill. 1955. Pp. ix+492. $6.00.

Those who know the 1948 edition will need to be told only that the book has been completely rewritte although the layout and chapter headings are similar; that there are completely new chapters on selectio and classification and on social factors in industry; that more than a quarter of the references are date 1948 and later.

Those who have not read the earlier edition will want to know a little more. To my mind this 195 edition is the best text-book on industrial psychology, for several reasons. First, it is well written, a topics are clearly set out, classified and defined. Secondly, it is scientific in temper, general statemen and advice strictly following on research findings, and claims and generalizations of others are alway submitted to critical assessment; logic is shown to be still a powerful tool. Thirdly, wisdom and sense a as cement between the separate sections of the book. Fourthly, it is up-to-date but with no trace finality about it; no impression is left on the reader that a third edition will be unnecessary.

Examples of the authors' manner and matter are as follows. 'Discussions of fatigue are likely to b characterized by circular reasoning which completely invalidates any conclusions that are drawn.' 'It not regarded as strange that a man will be motivated to avoid social contacts at breakfast time, while the evening he actively seeks such contacts.'

The new Ghiselli and Brown, then, is *the* book for students who need a good survey of what personn and industrial psychology is all about. But it is more than that: because of its scientific basis, it coul serve as an interesting introduction to general psychology. It is short on theory, but I could imagine head of a psychology department finding parts of this book as starting points, check points or illustra tions to a study of theory in (say) social psychology.

Such criticisms as this reviewer can make fall into two groups. One group were found, on examinatio to be not criticisms of Ghiselli and Brown at all but of the present state of psychological research. Fo example, the chapters on interviewing and on training in industry are rather thin. This is not the fau of the authors but simply a reflexion of the state of knowledge on these subjects. The second group criticisms can probably be ascribed to patriotism: there are plenty of quotations from early I.H.R.I and M.R.C. work, but not, perhaps, enough attention paid to more recent British research. However, th is rather an impertinent criticism of authors who cite nearly 400 references. DENIS MCMAHO

The Psychology of Occupations. By Anne Roe. New York: Wiley; London: Chapma and Hall. 1956. Pp. xii+340. 54*s.*

The home-made blunderbuss, schizilly fired from behind a pillar in the cloister, has become quite a thi in recent years. We have discovered that, provided we are careful to shoot a reasonable number victims—fairly well mixed, perhaps, but with no really troublesome nonsense about sampling—a coup of days of slightly chaotic field sport will probably yield sufficient good clean and cosy post-mortem fu to keep us and our students occupied for long enough. If we work busily—using appropriate method of course—out of the machines in our morgues will tinkle factors to suit all tastes, few or many, orthogon or oblique. Nowadays nearly everybody knows that only what goes in can come out. All the sam a factor's a factor. If we don't happen to like the look of it when we see it, we can always give it anoth push and interpret it.

So many seem to think. Is Dr Roe among them? Maybe: for in this book, which—shorn of its openi irrelevancies, and its final exhortations, called 'implications'—is a bold but unsuccessful essay in t classification of occupations, she succumbs in a big way to the temptation to pigeon-hole them accordi to ideas suggested by interest-inventory enthusiasts who appear to have got many of their initial questio wrong. True, she starts off with a conducted tour of some social anthropology; but this turns out to be little pointless. Then she brings in a lot about Maslow and his basic needs, perhaps because she herself greatly seized by one of them, the need for understanding, and is commendably keen to have a theor

lowever, this too is a flop, and the whole boiling is quietly thrown overboard before the discussion on assification begins. Poor Maslow.

What Dr Roe is really out to do is to introduce us to an old friend in some new clothes. She categorizes ccupations according to their 'group' and their 'level'. Groups of occupations are differentiated by eference to their 'primary *foci* of activity' (outdoor, business contacts, technology, service, and so on). he level of an occupation is determined by the skills it demands and the responsibilities it involves. n short, Dr Roe's scheme is built on the same basis as the one widely used in the Youth Employment ervice in this country.

But it is not a very satisfactory basis. We may hope that Dr Roe will soon become as tired of it as we re; that she will soon see that most occupations are far too mixed to allow of their being regarded as belonging' to this group or that group; and that she will shortly start thirsting for a classification based n something nearer real life than her question-begging primary *foci*. It must be said, however, that of its ind her scheme is good; it is better thought out than ours; and what she has to say about it will certainly ave to be taken into account by classifiers of occupations until somebody comes along with something ndamentally better. Her documentation is almost first-rate.

ALEC RODGER

he Psychology of Adjustment. (Second edition.). By L. F. Shaffer and E. J. Shoben, Jn. Boston: Houghton Mifflin; London: Constable. 1956. Pp. xxi+672. 42*s*.

or this second edition of his 1936 text, now extensively revised and rewritten, Shaffer has as his co-uthor E. J. Shoben. The book begins with a very good introduction to motivation, learning and conflict. his is possibly the best section of the book, written with an economy and clarity of style that serves as a elcome reminder that difficult ideas need not be couched in difficult prose. There follows a discussion of ie major adjustive mechanisms; adjustment by defence, by escape, by illness and so on. The next ction describes the way in which organic and cultural factors determine personality, and the techniques ed to describe and measure personality. This very useful section includes a chapter on Psychoanalysis. he book continues by describing how child-guidance clinics function, how psychotherapy works, and ncludes with a discussion of mental hygiene in the school, at work, and in the home.

It would be remarkable if so ambitious a book were above criticism and, quibbles apart, three points em important enough to deserve mention. The authors deal in a quite satisfactory way with the psycho-euroses, but the psychoses, organic brain lesions, the attack disorders and the toxic states meet with ant consideration. If these topics are outside the authors' terms of reference this should have been made ear. However, a text on the psychology of adjustment, howsoever defined, should grapple with the roblem of depression and here again only a casual reference or two to the subject is included. Yet the sychiatric clinics seem full of people who are depressed because they have failed to adjust, or have iled to adjust because they are depressed. Finally, their several references to treatment leave one with e impression that if the patient is neurotic the treatment will be psychotherapy, unless the illness is a sychosomatic one, when recourse to drugs or surgery is permissible. This over-simplification may accord ell with the authors' psychological analysis of maladjustment, but it leaves us with a distorted view of e work of the ordinary psychiatric clinic and mental hospital.

It will be seen that these criticisms refer to putative omissions. What has been said has been well said d well illustrated, and should provide an excellent introduction to the medical and non-medical udent of psychology who intends specializing in this field. The references are fairly recent and a gratifying mber of these cite British research.

JAMES DREWERY

he Social Psychology of George Herbert Mead. By Anselm Strauss (Ed.). University of Chicago Press; Cambridge University Press. 1956. Pp. xvi+296. 12*s*. 6*d*.

hen George Herbert Mead died in 1931 he left but few publications behind him. He did, however, ave papers and notes and from them a group of students and colleagues compiled three volumes: *ind, Self and Society, Movements of Thought in the Nineteenth Century*, and *The Philosophy of the Act*. this book, Strauss has brought together selections from all three. Running through them all is the nception of the Self as socially derived through the meaningful gesture, which induces its performer to ake on the attitude of the other'. This achievement of self-consciousness comes at the end of a long ocess of evolution, and Mead discusses the significance in philosophy of the evolutionary hypothesis. the beginning was the Act, and in action 'the living being determines its environment'. 'Mental

processes are just those phases of conduct into which the self as an object has come to deepen and rende significant our analysis and to make possible the rational solution of our problems.' Mead as pragmatis is represented in the sections on 'The Power of Mind' and 'Stages in the Act'. He was naturally intereste in the nature of scientific thought, and when he deals with history as a science he equally naturally take the line that all history is contemporaneous—the past is the setting for the present and changes with i Two sections are devoted to these matters.

These posthumous publications carry with them the defects of their origins. Since they were derive mainly from lecture notes, they are frequently obscure, sometimes incantatory, and all too often repetitiou These strictures apply least to *Mind, Self and Society* which is much the most coherent, and, indeed, delight to read. The selections from that book have been checked, and they have been skilfully mad The other two books were not available.

One complaint must be voiced. No references to the original volumes is made; we do not know fro what page or chapter the extracts have been taken. Perhaps Dr Strauss has not incorporated th 'generalized other' of the world of scholarship, but to those who have the ommission is exasperatin However, Mead is too little known in this country, and we must be grateful to Dr Strauss for making h work available in a compact form.

W. J. H. SPROT

Mental Health and Mental Disorder. By Arnold M. Rose. London: Routledge an Kegan Paul. 1956. Pp. xii+626. 40*s*.

In this book, Rose has collected thirty-eight papers from forty-six contributors. On the whole, it is a admirable piece of work and the editor and eight subeditors deserve our thanks for what is, undoubtedl the best available reference book on the sociological aspects of mental disorder. A quarter of the selectio were written especially for this volume, and the rest with some exceptions are reprinted as they appeare originally. Consequently, there is some overlapping and repetition. For example, the Chicago studies Faris and Dunham are referred to whenever the term 'ecological' occurs. These studies show that th incidence of mental disorder is highest at the centre of the city and declines in all direction towards th periphery. Since this finding is supported by similar studies carried out in nine other cities, it is a maj point in favour of examining environmental factors which contribute to mental disorder.

This is not the book to refer to for any evidence of hereditary factors in mental disorder—as the su title says, it is 'A Sociological Approach'. In fact the word 'heredity' seldom appears, and althoug Kallman does get into a footnote, his name is not included in the index. Nevertheless, several contributo do mention the possibility of constitutional predisposition to mental disorder, even though this is not th main purpose of their papers. Otherwise the book is remarkably free of any doctrinaire bias. Some of th early chapters support a Freudian viewpoint, but Sewell cautiously presents evidence that would lea us to question the hypothesis that infant training affects childhood personality in the way predicted b psychoanalysts. Green is frankly anti-Freudian or at least hostile to the Horney-Fromm deviation, bu his approach is hardly more than anecdotal.

Many different theories as to the causation of mental disorder will be found in this book. It is due to th failure of society to provide social roles, and the low degree of social organization in certain areas (Burgess neurosis is the result of a conflict of roles due to the conflicting socio-cultural environment—a lack integration between the primary and secondary selves (Dai). Primitive societies are relatively free fro mental disorder because they are static, whereas American society is rapidly changing (Burgess); ment disorder is prevalent in primitive societies (Demerath). According to Burgess, neurosis arises becau modern society is too complex—there are 50,000 different occupations to choose from—a view which later contested by Schermerhorn. In America, it is flexibility and social change which are the princip sources of insecurity, while in Europe stratification and rigidity result in frustration (Frumkin, quotin Ruesch). People must be helped to adjust to the modern world (Burgess) but 'normality' is a middl class ethic disguised as science (Schermerhorn). The debate continues—but each new speaker has n heard the previous speeches.

In this book there is plenty of well-intentioned preaching which will stimulate our sympathy but n our scientific curiosity; there is wild speculation, woolly thinking and dubious theorizing. But when the pages are eliminated, we are left with a rather smaller book which would still be well worth the price, a a constant source of data and references. Not all the papers are of equal value to the reader, but t 'Summary of Studies on the Incidence of Mental Disorders' (Rose and Stub) with its wealth of thoug provoking data would surely be counted as one of the ten essential contributions.

To summarize, the main purpose of the book is to challenge any complacency regarding our social esponsibility for mental disorder. To this end the book contains a theoretical discussion of Social sychiatry—emphasizing the contribution of psychoanalysis to sociology and, less emphatically, claiming sociological contribution in return; a detailed summary of various studies analysing the correlates of ental disorder in terms of age, sex and such social factors as economic status, occupation, residence, arital status and race—it is interesting to notice how consistently there are higher rates for foreign-born s compared with 'native-born' Americans; a section on mental disorder in various communities—rimitive, religious (the Hutterites), rural, semi-rural and cities; a sociological analysis of schizophrenia, sychopathy, drug and alcohol addiction; and some rather speculative papers on preventive and thera-eutic measures. The challenge will not meet with the same response from all readers, but the book is valuable to all who research or teach in the field of mental health and mental disorder.

GODFREY GARDNER

Vhen Prophecy Fails. By Leon Festinger, W. Riecken and S. Schachter. Minnesota University Press; London: Oxford University Press. 1956. Pp. vii+256. 32*s*.

is conceivable that this book may usher in a new genre—the mild psychological thriller written by and r professional psychologists. The bulk of it is an exciting, often astounding, narrative about a contem-orary group of Americans who predicted the coming of a second world-cleansing flood on a specified date, nd believed themselves to be an élite who would be picked up by flying saucers (on which numbered eats would be reserved for them) and taken to a benevolent planet where they would be trained to eturn to and repopulate the earth. The authors, all members at the time of the Laboratory for Research Social Relations of the University of Minnesota, displayed journalistic opportunism in insinuating cognito participant observers into the group shortly after the public announcement of the prediction nd in maintaining an extremely thorough coverage by these rather 'cloak and dagger' means till the roup dispersed after the failure of the prophecy.

Considered simply as an assiduously reported 'human interest' story, the book could hardly fail to ppeal to anybody with normal curiosity in the wilder extremities of human belief and the resulting ddities of behaviour. But it was of course all done with a serious scientific purpose, namely, to show how nd why it is that believers in a categorical prediction can emerge from its invalidation not only undaunted ut with even stronger conviction and greater missionary zeal. The result is a sandwich-type book, the arrative forming the generous portion of meat between thin slices of scientific bread. One slice is an troductory chapter briefly surveying the history of millennial and Messianic movements and stating the ypotheses which were to be tested by direct observation of this particular movement. The other slice is a isarmingly frank methodological appendix which is in many ways the book's most interesting feature.

But even when methodological doubts are set aside, it is difficult to feel convinced that the book's cientific objective has been completely achieved. The so-called 'hypotheses' seems to be inductive eneralizations based largely on the historical survey of millennial movements, and they are stated so as to pecify five conditions necessary for an increase in proselyting to follow an unfulfilled prophecy. The uthors concede, however, that failure of a prophecy to come true is sometimes followed by the complete reak-up of the movement, with no further attempts to convert others, even though the five conditions ave been satisfied. They do not pursue this damaging admission, possibly because it would surely lead hem into an analysis of the personality dynamics of individual members of the group, and they have hroughout studiously avoided the orthodox 'genetic' analysis in favour of a Lewinian group dynamics pproach. In consequence, it all reads like an odd case-history—odd because it concerns a group rather han an individual, and because practically all the stock motivational interpretation has been deliberately ft out. Such a self-denying ordinance seems only to make the task needlessly difficult and perhaps mpossible. At any rate, it is arguable that even a fictional version of the more usual motivational analysis ke Joyce Cary's account of the Adventists in his *Except the Lord*) brings more insight than this rather inkered approach. Perhaps this is not entirely fair comment, for the authors finally restrict themselves explaining 'why increased proselyting follows the disconfirmation of a prediction', and their explana-on is 'one derivation from a general theory' of dissonance which is to be elaborated in a forthcoming ook by Leon Festinger. Possibly, therefore, this book merely suffers in this respect from premature ublication.

Notwithstanding these misgivings, this is undoubtedly a field study of remarkable novelty and nothodoxy which brings several important questions into sharp focus.

A. J. M. FLOOK

Psychology, Psychiatry and the Public Interest. Edited by MAURICE H. KROUT. Minn
sota University Press, London: Oxford University Press. 1956. Pp. xv+217. 32

The relationship between clinical psychology and psychiatry is the main topic of this book. Its ai
according to the editor's preface, is peace, 'based on mutual understanding and mutual recognitio
which 'can come only from a broad and free discussion of the relationship between psychiatry and ps
chology by qualified representatives of both professions'. This being so, it is surprising to find that, of t
fifteen individual contributors, only one (Paul E. Huston) is a psychiatrist. The remaining contributo
are psychologists in the U.S.A., most of them working in a medical setting or in private practice. Eight
the chapters have already appeared elsewhere. This uneven balance of forces and the tone of some of t
chapters tend to belie the preface, and a good half of the book might well have borne the title 'The Case f
the Psychologist in Private Practice'. This does not mean that it has no relevance for British psychology.
the course of presenting the case, opinions are expressed on matters of great importance for both professio
here and elsewhere. Opinions, however, do not win arguments and the supporting evidence is scanty

A question on which there is much agreement between the psychologists and complete disagreeme
between them and the medical attitude as expressed in the book is, in the title of one chapter, 'Wh
Shall Practice Psychotherapy?' Many of the psychologists argue that the treatment of neurosis can
satisfactorily handled by an appropriately trained psychologist working without medical supervisio
Robert M. Lindner, to whose memory the book is dedicated, goes so far as to suggest, in a posthumo
reprint of an earlier paper, that present-day medical and psychiatric training does not produce pract
tioners competent to deal with psychopathological cases. 'Among the medical practitioners of psych
therapy of this generation', he writes, 'the majority are but spottily educated and informed. In the pla
of a broad systematized knowledge of man, his culture, and his works this majority possesses a handful
clinical facts wrapped about with terminological vagaries and ill-considered generalizations.' One ca
feel much sympathy for this point of view and yet have an uneasy feeling that its proponents are then
selves speaking from insecure positions. One looks in vain for evidence that either group is better tha
the other when it comes to helping the people in need.

A point which is made several times throughout the book, and which is used to justify the above pos
tion, is that neuroses and character disorders are *not* medical problems and that psychotherapy is *not*
medical procedure. The first chapter, by Nevitt Sanford, makes some interesting observations in th
connexion, and the point is developed by several contributors, particularly Carl R. Rogers. The idea th
many psychiatric patients are not really ill but are seeking help for the solution of problems of livi
which they feel unable to solve unaided must often strike those who work with neurotics. This is, perhap
the central problem posed by the book. It is an important problem and one which deserves a much mo
thorough examination than it is given by Dr Krout and his collaborators.

The authors have earned our gratitude for bringing into the open a number of important and controve
sial issues. It is to be hoped that somebody will now be stimulated to undertake the more difficult ta
of examining these issues more thoroughly in the light of all the evidence he can obtain.

J. G. INGH

Das autogene Training. (*Konzentrative Selbstentspannung.* Ninth edition). By J.
SCHULTZ. Stuttgart: Thieme, 1956. Pp. xx+351. D.M. 29.

Autogenes Training is understood as a systematic, concentrative relaxation without any specific sugg
tion. The aim is to achieve a general switching of the normal state of wakefulness to a generalized feeli
state of low awareness. This frees the subject from all his specific problems and induces states of warm
heaviness and relaxant feelings. A wide variety of accompanying physical and psychological concomitan
as well as the relationship to a number of related techniques, are discussed in detail. The method is,
individual or group administration, applied to a variety of disturbances of the vasomotor, respirato
digestive or central nervous systems. Thus, non-neurotic patients, psychoneurotics (with the exception
deep-rooted chronic states), and a variety of psychopathic personalities are said to benefit distinctly t
degree of 63%. If one compares the ninth with the first edition (1932), it becomes immediately clear th
neither method nor theory has changed to any considerable extent. In fact, no new data of an expe
mental kind have been added. Any other recent literature that has been considered is of the practi
clinical type. Anglo-American literature is only occasionally referred to, and no systematic referenc
made to the great number of experimental psychological investigations that exist in this field. It wo
appear to the reviewer that such an attempt might have avoided a certain staleness and sterility in
otherwise important and probably rewarding field of clinical research.

J. C. BRENGELMA

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